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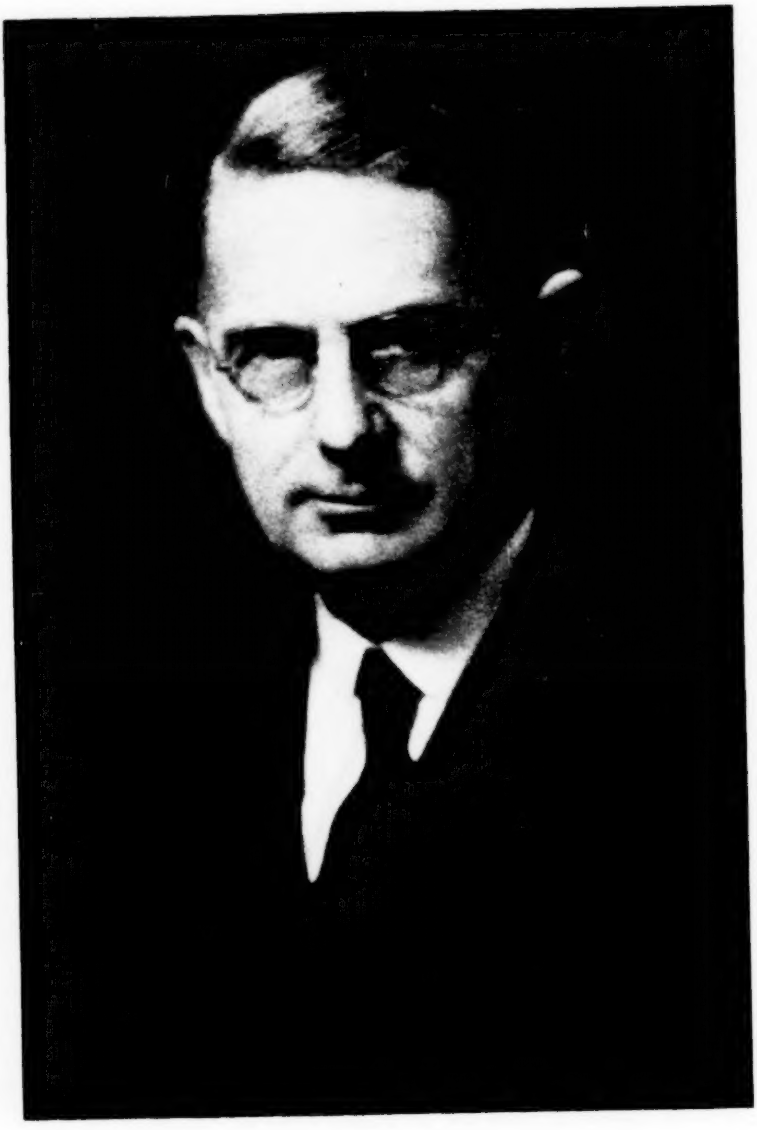

J. M. Clark.

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SOME NOTES ON MULTIPLIER THEORY

By RALPH TURVEY[1]

The theory of the multiplier may be viewed in either of two ways. It may be regarded as the specification of a set of relationships which the econometrist is to measure, or alternatively as of purely qualitative significance, its purpose being to explain one aspect of the determination of national income. In the latter case its function is to display the relevance and *modus operandi* of various factors.

In either context, the tendency in recent writings has been to develop a less aggregative approach as, for example, in the discussion of matrix multipliers. The present article is intended as a further contribution in this direction, but it aims at synthesis as well as innovation.

The first section restates the static theory for an economy with a government and with foreign trade, and in order to achieve consistency with the social accounting approach makes a sharp distinction between firms and households. This involves a clear separation of business saving from personal saving, a good example of useful "disaggregation." Section II analyses the effects on national income of changes in government spending and taxation without being confined to one type of tax and one type of spending. It is shown that the theorem that a balanced budget-increase has a multiplier of unity is a very special case. Section III, which uses a pure Stockholm School approach, demonstrates how the relationship between saving and investment *ex post* and *ex ante* can only be understood if, consonantly with the division between households and firms, the markets for factors and for goods are distinguished. It is also shown that since saving (*i.e.*, not-spending) is a derived concept, analysis in terms of spending is preferable because of its directness.

[1] The author is reader in economics at the London School of Economics, University of London.

I. *The Static Multiplier*

The multiplier analysis of the determination of the level of national income is essentially a special case of general equilibrium theory, differing from it in respect of a number of simplifying assumptions.[2] For the purpose of this paper, the main assumptions may be given the following form:

1. Groups of economic units are consolidated into sectors which are treated as units, *i.e.*, the analysis is macro-economic.[3]
2. Only income effects enter into the behaviour equations.
3. Certain magnitudes, such as investment, are autonomously determined.
4. The supply curves of all newly produced goods and of factor services are infinitely elastic; thus no distinction is made between the real and money values of the variables.

The analysis is, to start with, in terms of comparative statics and relates to the short period, though one aspect of growth economics is briefly mentioned.

The simplest way of describing the system is to list the transactions which enter into the analysis, as in the table below. Since this table is largely self-explanatory, little need be said about it. Items not shown, such as profits tax and wage payments by government are assumed to be zero. Indirect Taxes (and Subsidies) are on consumer goods only,

Paying Sector / Receiving Sector	Firms	Households	Government	Rest of the World
Firms	Interfirm transactions in intermediate & capital goods	Consumption (at market value)	Government Purchases Subsidies	Exports
Households	Factor Purchase (= Factor Sale)		Transfers	
Government	Indirect Taxation	Income Tax		
Rest of the World	Imports			

[2] Compare Burgess Cameron's derivation of the Leontief system from general equilibrium theory. "The Construction of the Leontief System," *Rev. Econ. Stud.* (1950-51), XIX (1), 19-27.

[3] Richard M. Goodwin "The Multiplier as Matrix," *Econ. Jour.* (Dec. 1949), LIX, 537-55 and John S. Chipman, *The Theory of Intersectoral Money Flows and Income Formation* (Baltimore, 1951) provide a formal analysis of *n* sector models.

and are paid (received) by Firms which are also assumed to handle all Imports. The term Factor Purchase is used to include all income payments by Firms to Households: wages, salaries, rent, interest and dividends.

Neither National Income nor Investment[4] is shown explicitly, the former because it is a derived concept, and the latter because it consists of several components, one of which, physical increases in Firms' stocks of their own products valued at market price, is only a payment flow to the extent of the necessary Factor Purchase. Investment being that part of the national output not sold by Firms to other sectors, the relationship between it and National Income is given by the following identity:

$$Y \equiv I + G + E + C + L - K - M \quad (1)$$

Y	I	G	E	C	L	K	M
National Income (output) at factor cost	Investment	Government Purchases	Exports	Consumption	Subsidies	Indirect Taxation	Imports

National Income exceeds the wages, salaries, rent, interest and dividends received by Households (Factor Sale = Factor Purchase) by the amount of gross business saving.[5] The Disposable Income of Households is related to their Factor Sale as follows:

$$D \equiv F + T - U \quad (2)$$

D	F	T	U
Disposable Income	Factor Sale (Purchase)	Transfers	Income Tax

In order to explain the determination of National Income it is now necessary to specify the "spending functions" of the various sectors:

Rest of the World

The value of Exports (E) is taken to be exogenously determined.

Government

Government Purchases (G) of newly produced goods from Firms is assumed to be constant. Subsidies and Transfers are dealt with below.

Households

$$C = \alpha_3 + h \cdot D \quad (3)$$

C	α_3	$h \cdot D$
Consumption at market value	constant term	Households' Disposable Income

[4] Both of which are taken gross throughout.

[5] Plus corporate taxation. This is assumed to be zero in this section of the paper.

h is the marginal propensity of Housholds to consume with respect to their Disposable Income.

$$\underset{\substack{\text{Income}\\ \text{Tax}}}{U} - \underset{\text{Transfers}}{T} = \underset{\substack{\text{constant}\\ \text{term}}}{\alpha_4} + \underset{\substack{\text{Factor}\\ \text{Sale}}}{t \cdot F} \qquad (4)$$

where t is the marginal rate of Income Tax net of Transfers. The constant term will be negative, since at low income levels Transfers will exceed Income Tax and at high income levels Income Tax will exceed Transfers which may fall as Factor Sales and employment rise.

Firms

Firms pay Indirect Tax and receive Subsidies on their sales of consumer goods. Assuming proportionality:

$$\underset{\substack{\text{Indirect}\\ \text{Taxation}}}{K} - \underset{\text{Subsidies}}{L} = \underset{\substack{\text{Consumption}\\ \text{at market}\\ \text{value}}}{k \cdot C} \qquad (5)$$

k is the marginal (and average) rate of Indirect Taxation minus Subsidies expressed as a fraction of Consumption at market value.

This, and equations (3) and (4) involve an aggregation problem. Thus a simple relation between Consumption and the Disposable Income of Households requires either that changes in the distribution of the latter have no effect on Consumption (all Households have the same h) or that the distribution is a function of the level of Disposable Income.

The aggregation of all firms into one sector is more complicated, because they not only make purchases from other sectors but also purchase capital goods and intermediate goods (materials) from one another. Consider first a single firm. Apart from Indirect Taxes, its purchases (payments) may be listed as follows:

(a) Intermediate goods from other firms or the rest of the world. Some of these purchases will vary with output while others, *e.g.*, purchase of fuel for heating and stationery for the office, will be overheads. In addition a firm may invest by increasing its stocks of intermediate goods.

(b) Capital goods from other firms or the rest of the world. Given both the total investment of the firm and its composition, this item will be constant.

(c) Rent and interest paid to Households. The analysis being short-term, these overheads are assumed constant.

(d) Wages and salaries paid to Households, whether overheads such as payments for management and research (assumed constant) or variable labour cost.

(e) Dividends paid to Households. These depend upon profits which, given (a) to (d) above, are a function of sales.

The investment carried out by a firm consists of three components: purchase of newly produced capital goods,[6] purchase of intermediate goods for stock and increase in stocks of its own product, this last component being the difference between output and sales. The items (a) to (e) above can all be taken to be functions of Sales + Investment on the assumption that the composition as well as the magnitude of Investment is constant. Thus the Factor Purchase, Import Purchase and Purchase of Goods from other firms of any firm are functions of the sum of the Sales of its product and its Investment.

The aggregation problem[7] consists of deriving from all these functions two functions relating aggregate Imports and aggregate Factor Purchase to the sum of aggregate Investment and aggregate Sales (by all firms together) of newly produced goods to all other sectors. Assuming linearity[8] the two functions can be written:

$$\underset{\text{Imports}}{M} = j\cdot(\underset{\text{Investment}}{I} + \underset{\substack{\text{Government}\\ \text{Purchases}}}{G} + \underset{\text{Exports}}{E} + \underset{\substack{\text{Consumption}\\ \text{at factor}\\ \text{cost}}}{C-K+L}) \tag{6}$$

$$\underset{\substack{\text{Factor}\\ \text{Purchase}}}{F} = \underset{\substack{\text{constant}\\ \text{term}}}{\alpha_7} + b\cdot(I + G + E + C-K+L) \tag{7}$$

The constant term is equal to overheads (other than depreciation) plus any constant term in the dividend payment function. The significance

[6] From the point of view of the individual firm purchases minus sales of second-hand capital goods should be included, but for the economy as a whole these items cancel out.

[7] It may well be that the aggregate functions are derivable only on the assumption that all firms are identical, *i.e.*, in a one-commodity world. Certainly it will not do to assume that the proportional composition of final output and of the inputs of each separate firm is invariant with respect to the level of output, for that would be inconsistent (a) with the treatment of some inputs as overheads which are fixed in the short run and (b) with the assumption that I, G and E are given.

Goodwin maintains (*op. cit.* pp. 541-43) that the Keynesian system implicitly assumes that the marginal propensity to save of all firms is zero so that if output increases by x total Factor Purchase will increase by x whatever the composition of the increase in output. (This relates to a closed economy.) He rightly says that this procedure has the disadvantage of ignoring "the phenomena of internal financing, of the failure to disburse earnings, of heavy fixed charges and of the payment of dividends above current earnings" (p. 543).

[8] Even if this is not an approximation there is the additional complication that straight-line relationships may be kinked. It is reasonable, for example, to assume that dividends equal half net profits, but unreasonable to suppose that when net losses are being made shareholders will pay money to their firm!

of b and j can best be illustrated by an example: Suppose that when output and sales rise by £100 an additional £50 must be spent on labour and £10 on imported raw materials and that of the £40 increase in profit half is paid out as additional dividends. Then $b = 0{\cdot}7$ and $j = 0{\cdot}1$. It will be seen that $(1 - b - j)$ is the marginal propensity of firms to save with respect to (Investment + Sale of Goods to other sectors).[9] $(b + j)$ is not the marginal cost of final output to the sector of Firms, since it refers to money value not to quantity. It is not even the marginal cost of £1 worth of output, since it includes marginal dividend payments and the import of finished goods as well as materials.

Solution of the seven equations for National Income (Y) gives:

$$Y = \frac{I + G + E + (1 - k) \cdot [\alpha_3 + h \cdot \{-\alpha_4 + (1 - t) \cdot \alpha_7\}]}{1 - b \cdot (1 - t) \cdot h \cdot (1 - k)} (1 - j) \tag{8}$$

The numerator, the multiplicand, consists of the "injections" Investment, Government Purchases and Exports together with the invariant part of Consumption (at factor cost) due to the constant terms in the Consumption, Tax-transfer and Factor Purchase functions—all multiplied by $(1 - j)$ to remove the import content.

The denominator can be explained as follows: Of an increase in Investment or Sale of Goods, b is paid to Households and t of this is taken in Income Tax. Households spend h of what is left on Consumption, the value at factor cost being $(1 - k)$ of this.

A geometrical version of the multiplier is given in Figure 1 where the dashed lines show the equilibrium position. The N.E. quadrant shows Consumption at market value as a function of Disposable Income (3)[10] and Indirect Taxation minus Subsidies as a function of Consumption expenditure (5). The N.W. quadrant adds Consumption at factor cost to Investment, Exports and Government Purchases (1). The S.W. quadrant shows their sum as determining Factor Purchase and Imports (7) and (6). The S.E. quadrant relates Disposable Income to Factor Sale (2) and (4).

The multiplier formula given above (8) differs from the more customary ones in that, consonantly with the clear division of the economy into sectors, the functions relating Factor Purchase to total spending and Consumption to Disposable Income are sharply distinguished instead of being lumped together into one consumption function, the orthodox consumption function, which fails to distinguish Firms behaviour from Household behaviour. The relationship of the

[9] Equals National Income plus Imports.

[10] The figures in brackets refer to the numbers of the equations.

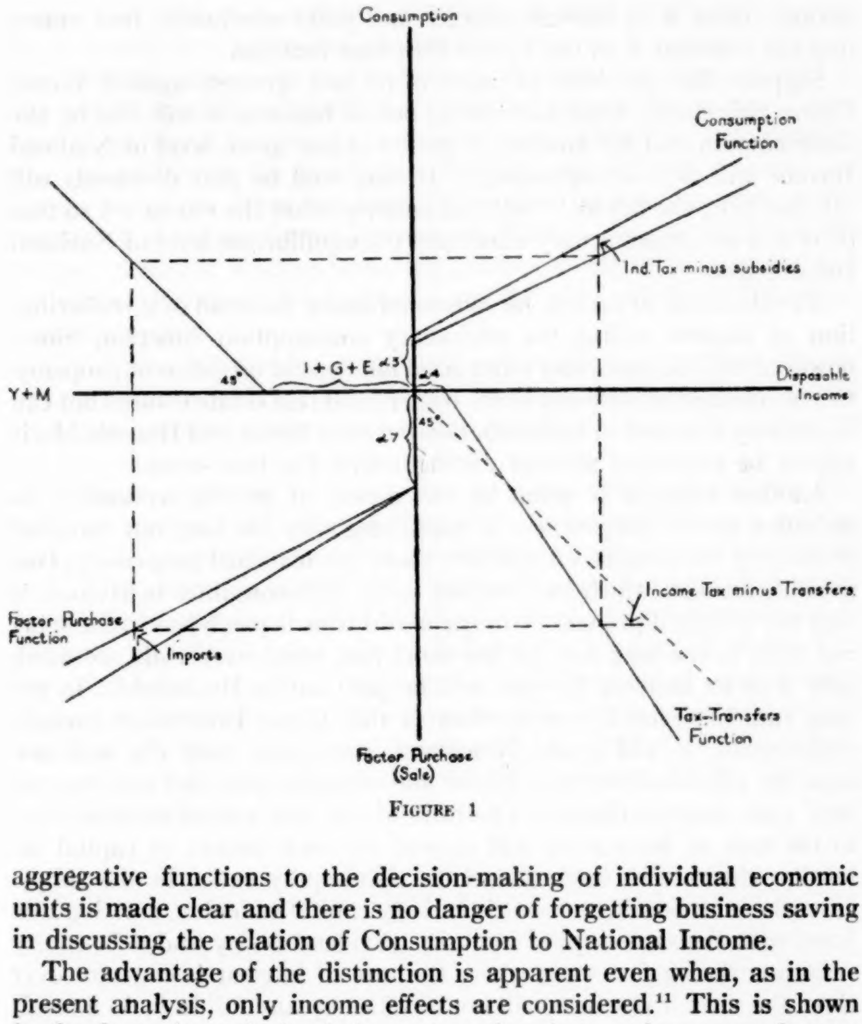

FIGURE 1

aggregative functions to the decision-making of individual economic units is made clear and there is no danger of forgetting business saving in discussing the relation of Consumption to National Income.

The advantage of the distinction is apparent even when, as in the present analysis, only income effects are considered.[11] This is shown in the dynamic analysis of the next section, but a minor example may be given here. In modern aggregative economics rent seems to have disappeared, widening the division between macro- and micro-theory. This is quite unnecessary, for rent (in the sense of periodic contractual payments for the use of real property owned by Households) is simply a payment from Firms to Households,[12] which is constant in the short

[11] The distinction is essential in the analysis of inflation. See my paper "Some Aspects of the Theory of Inflation in a Closed Economy," *Econ. Jour.* (Sept. 1951), LXI, 531-43.

[12] Or from some Households to others. This is not considered here since it merely redistributes income between Households, and such redistribution is assumed to have no effect on total Consumption.

period. Along with interest charges and other overheads, rent enters into the constant α_7 in the Factor Purchase function.

Suppose that the level of rents is revised upwards against Firms. Unless this causes some firms to go out of business α_7 will rise by the same amount and the amount of profits at any given level of National Income will fall correspondingly. It may well be that dividends will fall less (*i.e.*, the fall in b[13] will not entirely offset the rise in α_7) so that there is a net expansionary effect and the equilibrium level of National Income rises.

All this could, of course, be subsumed under the head of a redistribution of income raising the customary consumption function. Since, however, this expansionary effect need not depend on different propensities to consume as between share-owners and real estate owners but can be entirely a matter of redistribution between Firms and Households, it cannot be explained without distinguishing the two sectors.

Another example is given by one aspect of growth economics: in orthodox terms, the problem of explaining why the long-run marginal propensity to consume exceeds the short-run marginal propensity. One possible answer, which has nothing to do with consumer behaviour, is that the marginal propensity to spend of Firms is much less in the short run than in the long run. In the short run, when overheads are fixed, only b of an increase in Sales will be paid out to Households. In the long run, however, if output rises so that Gross Investment exceeds replacement, α_7 will gradually expand, since rents may rise and new capacity will have overhead labour and material costs and may have to bear some interest charges. The ratio of the time rate of increase in α_7 to the rate of investment will depend on such factors as capital intensity, the rate of interest and the debt-equity ratio. In the simple case where all new investment is entirely capital widening, creating new firms with the same capital intensity, dividend policy and debt-equity ratio as existing firms and where the rate of interest is constant over time, the secular rise in α_7 will not be offset by any fall in b. As a result the Factor Purchase function (and hence the orthodox consumption function) will show a secular upward movement, even though the Consumption-Disposable Income function (3) be unchanged.

II. *The Effects of Budget Changes*

The multiplier analysis will now be used to analyse the income effects of changes in the government's budget, both on the revenue and expenditure side.

[13] b may fall not only because of a decrease in the profits-output function but also because of a fall in the dividends-profit function, since the rise in overhead costs may increase the desire of firms to accumulate reserves.

In order to do this it is first necessary to point to an ambiguity in the meaning of a "given budget change." Suppose, for example, that t, the marginal rate of Income Tax net of Transfers, is raised so that at the original equilibrium position the yield of Income Tax minus Transfers is increased by £x. As is evident from Figure 1 and equation (8) this will lower the equilibrium level of both National Income and Factor Purchase. Since the latter determines Income Taxation minus Transfers, it follows that in the new equilibrium their net increase will be less than £x. Furthermore, the other consequential changes will include a fall in the cost of Subsidies and the yield of Indirect Taxation. More generally:

1. Unless a change in government expenditure or taxation has no effect on the variable of which it is a function, the impact change will equal the ultimate change[14] only if the marginal rate of expenditure or tax is zero.

2. Unless all other marginal rates of government expenditure and taxation are zero, the ultimate change in the budget deficit or surplus will equal the ultimate change in the expenditure or tax which is altered only if the alteration has no effect on the variables of which these other expenditures and taxes are functions. Suppose, to give an algebraic example, that the marginal rate of Indirect Tax is raised from k to k',[15] Subsidies being zero. If C_0 is the level of Consumption at market value in the initial equilibrium, the impact increase in the yield of Indirect Taxation will be $(k' - k) \cdot C_0$. Given unchanged values of Investment, Exports and Government Purchases, the resulting fall in National Income will equal:

$$\frac{(k' - k) \cdot C_0 \cdot (1 - j)}{1 - b \cdot (1 - t) \cdot h \cdot (1 - k')}$$

causing a fall in Consumption at market value $b \cdot (1 - t) \cdot h$ times as great, so that the ultimate change in the yield of Indirect Taxation will be:

$$(k' - k) \cdot C_0 \cdot \left(1 - \frac{(1 - j)}{1 - b \cdot (1 - t) \cdot h} \cdot b \cdot (1 - t) \cdot h \cdot k'\right)$$

The fall in the Budget Deficit, or rise in the Budget Surplus, will be less than this by the fall in the yield of Income Tax minus the rise in Transfers,

[14] In the sense of the difference between its value in the initial equilibrium and in the new equilibrium.

[15] The following results can be obtained very simply with the diagram by lowering the lower of the two curves in the N.E. quadrant and finding the new equilibrium position.

$$-\Delta U + \Delta T = -\frac{(k - k')\cdot C_0\cdot (1 - j)\cdot b}{1 - b\cdot(1 - t)\cdot h\cdot(1 - k')}\cdot t$$

The net effect, however, must be to decrease a deficit or increase a surplus; it is impossible for the fall in Income and Consumption to offset the rise in tax rate completely. This can easily be shown. The definitions:

$$\underset{\text{Business Saving}}{S_B} \equiv \underset{\text{National Income}}{Y} - \underset{\text{Factor Purchase}}{F}$$

and

$$\underset{\text{Household Saving}}{S_H} \equiv \underset{\text{Disposable Income}}{D} - \underset{\text{Consumption at market value}}{C}$$

combined with equations (1) and (2) give:

$$\underset{\text{Investment}}{I} + \underset{\text{Exports}}{E} + \underbrace{G+T+L-U-K}_{\text{Budget Deficit}} \equiv \underbrace{S_B+S_H}_{\text{Total Saving}} + \underset{\text{Imports}}{M}$$

By assumption I, E and G are constant, so, taking the differences between the initial equilibrium and the new equilibrium:

$$\Delta T+\Delta L-\Delta U-\Delta K=\Delta S_B+\Delta S_H+\Delta M$$

Since Income falls, so will Saving and Imports, and the left-hand side of this equation must therefore have a negative value too, which means a smaller deficit or bigger surplus.

A generalized analysis of the multiplier effect of budget changes can now be given.

Government expenditures can be classified according to their direct income-creating effect, and an "income-creating coefficient" whose value will be in the range $(0 \rightarrow +1)$ can be attached to each type. For example the purchase of newly produced goods with no import content will have a coefficient of unity, while the coefficient for tax-free Transfers will be the marginal propensity to consume of the recipients multiplied by $1 - k$. The coefficient for government purchase of land will be zero or almost zero since this is a sale on capital account.

Similarly, an "income-destroying coefficient" whose value will be in

the range ($-1 \rightarrow 0$) can be attached to each type of government revenue.[16] Thus the coefficient for death duty will be almost zero, and that for indirect tax on goods with no import content will be minus unity if there is money illusion in the consumption function.[17]

With the use of these coefficients the analysis falls into four stages:

1. Calculate the change in each type of expenditure and each type of revenue at the initial equilibrium position.
2. Multiply each such change in expenditure by its (positive) income-creating coefficient and each such change in revenue by its (negative) income-destroying coefficient.
3. Obtain the algebraic sum. This is the multiplicand.
4. Multiply by the multiplier calculated with the new marginal tax and expenditure rates.

It is evident that there are many possible combinations of initial and ultimate change in the deficit or surplus and change in National Income. Consider the set of cases where the budget change is confined to one revenue item and one expenditure item and their impact changes are equal in magnitude and sign so that initially the Budget Deficit or Surplus is unaltered. The multiplicand equals this change multiplied by the algebraic sum of the relevant pair of income-destroying and income-creating coefficients. It obviously may be negative, zero or positive.

In conclusion the special cases where the algebraic sum of the two coefficients is equal to the reciprocal of the multiplier may be noted. Here the change in National Income will equal the change in the budget; the multiplier of a balanced budget change is unity. One of these special cases, illustrated in Figure 2, has received considerable attention of late.[18] Assuming that: (1) Household saving is the only

[16] Compare G. L. S. Shackle, "The Deflative or Inflative Tendency of Government Receipts and Disbursements," *Oxford Econ. Papers*, No. 8 (Nov. 1947), pp. 46-64, where he constructs "inflative and deflative indices," both for full-employment conditions and for under-employment conditions. These are not the same as the coefficients defined above.

[17] As is assumed throughout this chapter; *i.e.*, it is supposed that the function relating Consumption expenditure to money Disposable Income is not shifted when a change in Indirect Tax or Subsidy rates alters the price-level of consumer goods. E. Cary Brown in his paper "Analysis of Consumption Taxes in Terms of the Theory of Income Determination," *Am. Econ. Rev.* (Mar. 1950), XLI, 74-89 shows that even "in the absence of a money illusion, consumption taxes are more effective, dollar for dollar, in cutting back consumer outlays than are income taxes—and this conclusion holds even though everyone be assumed to have an identical marginal propensity to consume. However they are less effective than when a money illusion is present" (p. 85).

[18] See the bibliography given in P. A. Samuelson, "The Simple Mathematics of Income Determination," *Income, Employment and Public Policy* (New York, 1948), p. 140; and Jørgen Gelting, *Finansprocessen i det ØKonomiske Kredsløb* (København, 1948), where four balanced budget multipliers are distinguished.

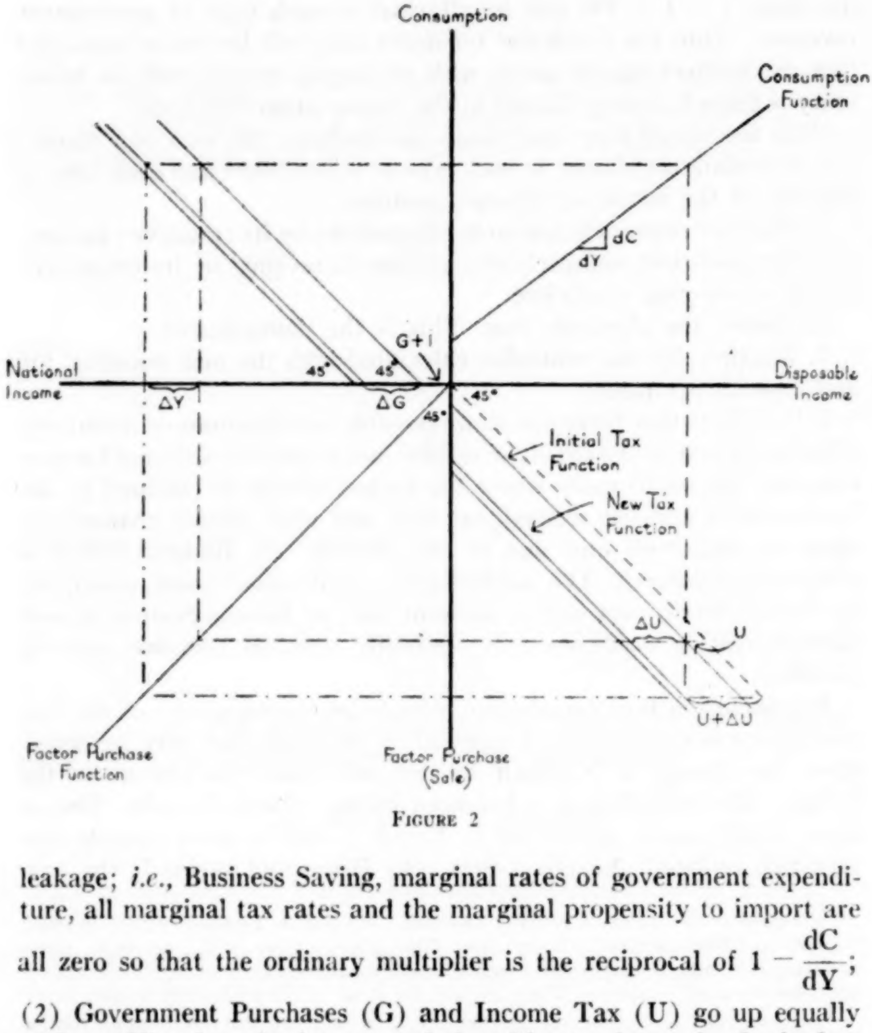

FIGURE 2

leakage; *i.e.*, Business Saving, marginal rates of government expenditure, all marginal tax rates and the marginal propensity to import are all zero so that the ordinary multiplier is the reciprocal of $1 - \frac{dC}{dY}$; (2) Government Purchases (G) and Income Tax (U) go up equally ($\Delta G = \Delta U$); then the impact and the ultimate change in the budget will both equal the change in National Income (ΔY) since the algebraic sum of the two relevant coefficients will be $1 - \frac{dC}{dY}$. This particular special case loses its paradoxical flavour when viewed in the light of a more general analysis.

III. *Some Aspects of the Dynamic Multiplier*[19]

For the purposes of this section, which is concerned with such matters as the multiplier time period and the relation between Saving and Investment during a multiplier process, the most useful method of analysis is that of the Stockholm School with its distinction between prospective (*ex ante*) and retrospective (*ex post*) magnitudes.[20] If the variables of the analysis are to be magnitudes rather than schedules, it is necessary to use what Lindahl calls the "disequilibrium method" where the prices of both goods and factors are fixed and made known at the beginning of each period when purchasers make their plans. The period, which is the interval between the making or revising of plans on the basis of expectations formed in the light of past experience, will be assumed to be coterminous for all Firms and Households.

In order to concentrate on essentials, this section is in terms of a two-sector model without a budget or foreign trade. The equations of the previous section are thus much simplified, the static model becoming:

$$\underset{\substack{\text{National}\\ \text{Income}}}{Y} \equiv \underset{\text{Investment}}{I} + \underset{\text{Consumption}}{C} \tag{1'}$$

$$\underset{\text{Consumption}}{C} = \underset{\substack{\text{constant}\\ \text{term}}}{\alpha_3} + \underset{\substack{\text{Factor}\\ \text{Purchase}\\ (=\text{Factor Sale})}}{h \cdot F} \tag{3'}$$

$$\underset{\substack{\text{Factor}\\ \text{Purchase (Sale)}}}{F} = \underset{\substack{\text{constant}\\ \text{term}}}{\alpha_7} + b(\underset{\text{Investment}}{I} + \underset{\text{Consumption}}{C}) \tag{7'}$$

so that given, as before, the magnitude and composition of Investment,

$$Y = \frac{I + \alpha_3 + h \cdot \alpha_7}{1 - b \cdot h}. \tag{8'}$$

For such a closed economy without a budget, the condition of aggregative equilibrium, in the sense that plans shall be fulfilled and expecta-

[19] Parts of this and the first section incorporate, in a revised form, most of the argument of "The Factor and Goods Markets" by Ralph Turvey and Hans Brems, *Economica* (Feb. 1951), XVIII, 57-68. Brems' contribution is to be found in Ch. 4 of his book *Reklame, Købelyst og Købeevne* (Copenhagen, 1950), and he has provided a restatement in his article "Ligevaegt, Forventninger og Planer: Makromodellen," *Nationaløk. Tids.* (1951), LXXXIX, 234-55.

[20] On the methodology of Swedish period analysis see Erik Lindahl, *Studies in the Theory of Money and Capital* (London, 1939), Part I and my chapter "Period Analysis" contributed to W. J. Baumol, *Economic Dynamics: An Introduction* (New York, 1951).

tions realised, is usually stated as the equality of prospective[21] Investment and Saving. The significance and limitations of this condition can only be fully understood by relating it to conditions in the goods market on the one hand and in the factor market on the other hand.[22]

Investment, Business Saving and Household Saving can be defined as follows:[23]

Investment ≡ Purchases of Capital Goods
+Purchases of Intermediate Goods for stock
+Output of all Goods } = market value of increase in Firms' stocks of
−all Sales of Goods } their own products

Business Saving ≡ Output of all Goods } = value added
−Purchases of Intermediate goods for production }
−Purchases of Factors } = all payments by Firms to Households, including dividends

Household Saving ≡ Sales of Factors
−Purchases of Consumption Goods

This gives:

Investment-Saving ≡ all Purchases of Goods
−all Sales of Goods
+Purchases of Factors
−Sales of Factors

Using the terminology of Bent Hansen who was the first to extend and apply this analysis,[24] this can be rewritten:

Investment − Saving ≡ Goods Gap + Factor Gap

These identities hold both prospectively and retrospectively. Prospectively a gap is the excess of the value of planned purchases over the value of expected sales. Retrospectively, since every actual purchase corresponds to an actual sale, both gaps are of course zero and Investment is identically equal with Saving.

[21] *Prospective* is used to mean *ex ante,* planned, intended or expected; *retrospective* to mean *ex post,* realised or actual.

[22] The following paragraphs on this relationship are based on Grünbaum's "Inkongruente Forventninger og Begrebet Monetaer Ligevaegt," *Nationaløk Tids.* (1945), LXXXIII, 100-18, and part V of Tord Palander, "Om 'Stockholmkolans' Begrepp och Metoder," *Ekon. Tids.* (Mar. 1941), XLIII, 88-141, which will appear in English translation in no. 3 of *Internat. Econ. Papers.* See also E. Schneider, "Saving and Investment in a Closed Economy," *Internat. Econ. Papers* (1951), no. 1, pp. 84-103.

[23] Investment, Saving and Income are taken gross throughout.

[24] See his book *A Study in the Theory of Inflation* (London, 1951), which (*inter alia*) discusses the Factor and Goods markets and the Saving-Investment relation in conditions of suppressed inflation. He uses the word "commodity" where this article used the word "good."

Since equilibrium in the goods market requires a zero Goods Gap and equilibrium in the factor market requires a zero Factor Gap, it is evident that the equality of planned Investment and intended Saving is a necessary, but *not* sufficient condition of aggregative equilibrium in the sense of congruency of plans and expectations.[25]

Suppose, for example, that though prospective Saving and Investment are equal there is a positive Goods Gap and an (equal) negative Factor Gap. If all purchase plans are realised the actual results of the period will be that Firms have sold more than they expected, thus running down stocks, and Households have received a smaller Disposable Income than they expected. As a result, Firms will probably plan to increase their output in the next period and plan a corresponding increase in their Factor Purchase. Households, on the other hand, may lower their Factor Sale (income) expectations and plan a corresponding reduction in their Consumption expenditure. The following period, therefore, will almost certainly not be one of equilibrium; indeed, if the adjustments made by Firms and Households are sufficiently marked, there may be a negative Goods Gap and a positive Factor Gap: a reversal of conditions in the first period. Thus the initial prospective equality of Saving and Investment means very little.[26]

Again, suppose that expected Sales of both goods and factors are above their equilibrium levels and that there is an excess of prospective Saving over Investment. This situation is quite compatible with the existence of a positive Goods Gap if the (negative) Factor Gap is larger. If all purchase plans are realized, the result will be that Firms

[25] For other reasons why prospective equality may not indicate equilibrium see: F. A. Lutz "The Outcome of the Saving-Investment Discussion," American Economic Association, *Reading in Business Cycle Theory* (Philadelphia, 1944), pp. 141-42; Grünbaum, *op. cit.*, sec. 5.

[26] Alvin Hansen in his paper "The Robertsonian and Swedish Systems of Period Analysis," *Rev. Econ. Stat.* (Feb. 1950), XXXII, 24-29, argues that analysis in terms of planned Saving and Investment is inadequate because "income" may mean the income of consumers or the sales by entrepreneurs, but he fails to provide an adequate analysis in terms of the factor and goods markets. Instead, he states on page 28 that "The condition of equilibrium requires *both* that intended investment shall be equal to designed saving *and* that actual investment shall be equal to intended investment. Or what comes to the same thing, the condition of equilibrium requires *both* that intended investment shall be equal to actual investment and *also* that desired (or designed) saving shall be equal to actual saving." This is formally correct, but not very interesting since this condition only indicates, at the *end* of a period, whether there *has been* equilibrium instead of indicating at the *beginning* of a period whether there *will be* equilibrium—*i.e.*, whether plans and expectations are mutually consistent.

The same applies to Harold M. Somers' statement that "The equality of realized and planned saving is sufficient and necessary for an equilibrium of income over time" on page 534 of his "A Theory of Income Determination," *Jour. Pol. Econ.* (Dec. 1950), LVIII. His statement is a definitional relation, since if consumption plans are realised, unintended saving is identical with unexpected income.

sell more Goods than they had expected. Hence they may plan to expand output in the following period, even though a movement towards equilibrium would require a contraction of their output and Factor Purchase.

So far nothing has been said of the capital market;[27] that is, only those money flows have been mentioned whose recipients regard them as being on current account, and the rate of interest has implicitly been assumed to be kept constant by speculation or open-market operations. In order to bring the capital market into the analysis, suppose it to deal in "Securities," these being all things sold on capital account—bills, debentures, shares, real estate, second-hand furniture, etc. Now from Walras' Law, the demand for everything including money is equal to the supply of everything including money, at any arbitrary set of prices, prospectively as well as retrospectively:[28] *i.e.*,

Demand for Goods, Factors, Securities and Money ≡ Supply of Goods, Factors, Securities and Money

so:

Excess Demand for Goods, Factors and Money ≡ Excess Supply of Securities

which can be rewritten as:

Goods Gap + Factor Gap + Net Hoarding[29] − Credit Creation ≡ Excess Supply of Securities (9)

or:

Investment-Saving + Net Hoarding − Credit Creation ≡ Excess Supply of Securities (10)

In contrast to conditions in the factor and goods markets, it may be

[27] As H. G. Johnson complains in his note "The Matrix Multiplier and an Ambiguity in the Keynesian Concept of Saving," *Econ. Jour.* (Mar. 1952), LXII, 197-200.

[28] Compare Bent Hansen's similar presentation of these identities in his paper "Til Renteteorins Almindelige Oplaegning" in *Axel Nielsen: Til Minde* (Copenhagen, 1951). His follow-up is different, however.

J. R. Hicks, *Value and Capital* (Oxford, 1946), Ch. 14, note A, p. 183, says "the difference between planned saving and planned investment is the difference between the planned demand and planned supply for securities in general—including money"; but argues that because of the interdependence of the whole system the "relating of particular equations to particular prices becomes rather idle." This is true for the Hicksian "week"—what Lindahl (*op. cit.*, p. 60) calls the second equilibrium method—but not for the present analysis which uses the disequilibrium method for the factor and goods markets and the first equilibrium method (see *ibid.* p. 65) and my chapter "Period Analysis" in Baumol, *op cit.* for the capital market.

[29] Defined as net increase in total money holdings of Firms and Households together.

assumed that in the capital market price is determined during the period by the demand and supply of securities which are thus brought into equality. The identity (10) just derived indicates that this implies that Investment and Saving together with Hoarding and Credit Creation must now be treated as schedules (with respect to the price of securities) instead of magnitudes.[30] Consequently the divergence between prospective and retrospective values of the variables can only be expressed with reference to the price of securities. For example, unintended Investment must now be understood as actual (retrospective) Investment minus the amount of investment planned at the security price determined during the period.

Since the price of securities equates the prospective demand and supply of securities, it can be seen:

1. From (10) that the schedules of Investment, Saving, Hoarding and Credit Creation can be said to determine the price of securities in each period. This is the loanable funds theory of the price of securities.

2. From (9) that, since the sum of the prospective values of: (a) The Goods Gap, (b) The Factor Gap, (c) Net Hoarding minus Credit Creation, is made zero through the working of the capital market, the condition of aggregative equilibrium can be stated as the requirement that any two of them shall be zero. It follows that neutral money (equality of prospective Net Hoarding and prospective Credit Creation) is not a sufficient condition for equilibrium.

3. From (9) that, if all purchase plans are realized and if actual Credit Creation equals its planned value,[31] the unintended Net Hoarding of Firms will equal their unexpected Sales (the Goods Gap) and the unintended Net Hoarding of Households will equal their unexpected income (the Factor Gap).

It has been assumed above that payment and purchase coincide—that there is no trade credit. If this assumption is relaxed, retrospective purchases of securities will exceed prospective purchases to the extent that Firms sell more goods and hence give more trade credit than they had expected. Allowance for this introduces considerable complications.

For the remainder of this paper the problems of the capital market will be ignored through the assumption that the price of securities is kept constant by open-market operations, so that Factor Purchase, Investment, etc., can be spoken of as magnitudes.

The objection raised here to analysis in terms of Saving and Investment is, of course, only an aspect of the aggregation problem. Just as a prospective equality of Saving and Investment may conceal a positive

[30] Unless their interest elasticity is zero.

[31] This will be the case if Credit Creation takes the form of the purchase of Securities.

Goods Gap and an equal negative Factor Gap, so may a zero Goods Gap conceal a negative Trombone Gap and an equal positive Ear Plug Gap. The point of the division between the factor and goods markets is that this is the most useful of the possible single divisions, at least for short-run analysis. In particular, this division is essential in order to make clear the relationship between prospective and retrospective Saving and Investment during a multiplier process. In order to demonstrate this the following assumptions will be made:

1. All purchase plans are realised, so that divergence between prospective and retrospective magnitudes is entirely a matter of unexpected Sales.

2. The planned Consumption of any period is a function of the Factor Sale expected; similarly the planned Factor Purchase of any period is a function of the sum of the expected Sales of consumer goods and planned Investment.

3. The Sales, of goods or factors, expected for any period equal the actual Sales of the preceding period.

Assumptions (2) and (3) mean that aggregative equilibrium implies constancy of National Income and its components and they make the Swedish treatment in terms of expectations and plans equivalent to models which have a simple one-period lag in the behaviour equations.[32]

The dashed lines in Figure 3 show an initial equilibrium assumed to exist in Period 0. Investment is I, Consumption OV, National Income (their sum) is OZ and Factor Purchase (equals Factor Sale) is OX.

Now suppose that beginning in Period 1 some Firms increase their rate of purchase of capital goods so that planned Investment rises from I to I′ and remains at that level in subsequent periods. This means that the curve in the N.W. quadrant, which adds planned Investment to expected Sales of consumer goods, shifts leftwards by I′ − I. Since, by assumption (3) the increased purchases of capital goods is not anticipated, the increase in planned Investment creates a Goods Gap of SZ. Since planned Factor Purchase and expected Factor Sale are still both OX, this Goods Gap is equal to the excess of planned Investment over intended Saving. Retrospectively, the Goods Gap will be closed and Investment made equal to Saving by unintended disinvestment (unexpected Sales) of SZ on the part of the Firms selling capital goods.

In period 2 the Sales expectations of Firms will be increased by SZ. Since planned Investment[33] is assumed to remain at I′ and Consumption has not yet increased, the Goods Gap is zero. Planned production will

[32] Similarly, the assumption of correct expectations is equivalent to an unlagged behaviour equation.

[33] Stock replacement is thus neglected. L. A. Metzler, "The Nature and Stability of Inventory Cycles" (*Rev. Econ. Stat.* [Feb. 1941] XXIII, 13-29), includes it in part of his analysis and also has a less simple assumption concerning expectations.

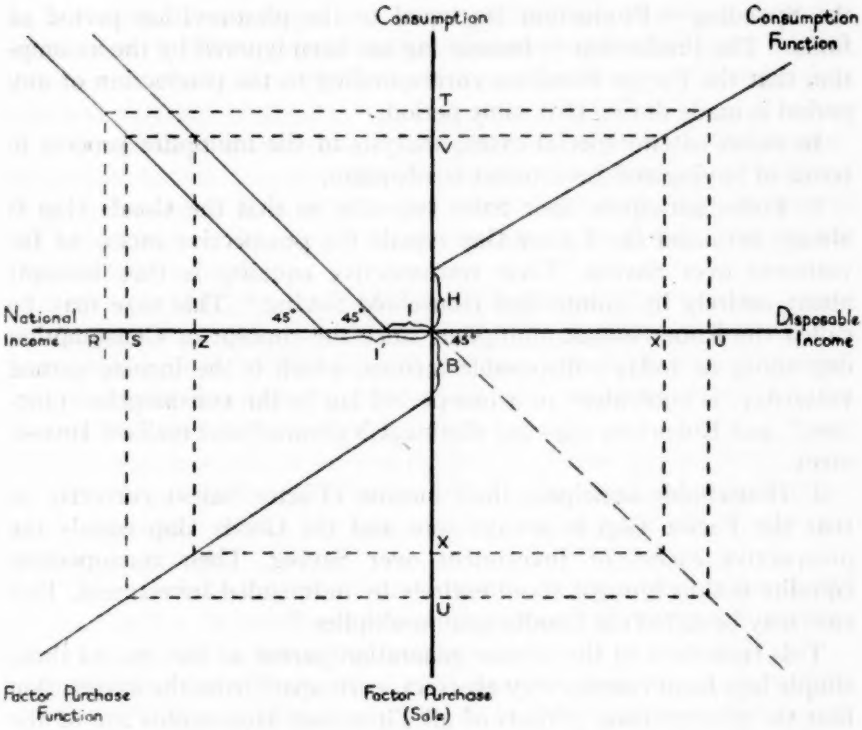

FIGURE 3

rise equally with expected Sales (by SZ) and planned Factor Purchase by *b* times this amount, that is by XU. Expected Factor Sales, however, are only OX (equal to actual Factor Sales in Period 1) so there is a Factor Gap of XU equal to the prospective excess of Investment over Saving. Retrospectively, the Factor Gap is closed and Saving raised to equal Investment by unexpected Factor Sales (Disposable Income) of XU, constituting unintended Saving.

Similarly in Period 3, the excess of Investment over Saving will be a matter of a Goods Gap of RS. Thus Period 3 is qualitatively the same as Period 1. This means that the income-generation period is two periods in length. What is the relationship between this and the customary statement[34] that the income-generation period is the sum of three lags: Income → Spending; Spending → Production; and Production → Income?

The answer is given by assumptions (2) and (3) above. As applied to Households they make the Income → Spending lag equal to the plan-revision period of Households, and as applied to Firms they make

[34] See Metzler's valuable paper "Three Lags in the Circular Flow of Income," *Income, Employment, and Public Policy* (New York, 1948), pp. 11-32.

the Spending → Production lag equal to the plan-revision period of firms.[35] The Production → Income lag has been ignored by the assumption that the Factor Purchase corresponding to the production of any period is made during that same period.

In either of two special cases, analysis of the multiplier process in terms of Saving and Investment is adequate:

1. Firms anticipate their Sales correctly so that the Goods Gap is always zero and the Factor Gap equals the prospective excess of Investment over Saving. Their retrospective equality is thus brought about entirely by unintended Household Saving.[36] This case may be called the Robertsonian multiplier, since the concept of Consumption depending on today's disposable income, which is the income earned yesterday, is equivalent to a one-period lag in the consumption function,[37] and Robertson does not distinguish planned and realised Investment.

2. Households anticipate their income (Factor Sales) correctly so that the Factor Gap is always zero and the Goods Gap equals the prospective excess of Investment over Saving. Their retrospective equality is thus brought about entirely by unintended Investment. This case may be called the Lundbergian multiplier.[38]

This treatment of the income-generation period as the sum of three simple lags is, of course, very abstract, even apart from the assumption that the plan-revision periods of all Firms and Households are of the same length and coincident. In particular, the Spending → Production lag can be treated as a simple lag only if all industry is fully integrated vertically.[39] While such complications destroy the simplicity of the two-sector model they do not, however, affect many of the conclusions reached with its aid. Thus a model with twenty goods markets would

[35] It is assumed here that the plan-revision periods of Firms and Households are of the same length.

[36] On the definitions used above there can be no unintended Business Saving if, as is assumed, all output and purchase plans are realised. A different assumption is made by Bent Hansen in his *A Study in the Theory of Inflation* (London, 1951), pp. 32-35.

[37] See Metzler's article on the three lags, *op. cit.*, pp. 17-18.

[38] Erik Lundberg, *Studies in the Theory of Economic Expansion* (London, 1937), Ch. 9, and Metzler's article on inventory cycles, *op. cit.* Both of these, however, bring in stock replacement in a later part of their analysis which is not the case in the present analysis, planned Investment being assumed constant. R. Eisner "The Invariant Multiplier," *Rev. Econ. Stud.* (1949-50), XVII (3), 198-202, points out that in this case National Income is always equal to actual Investment times the multiplier.

[39] Removal of this assumption requires a many-sector model. The necessary mathematics have been provided by Richard M. Goodwin, *op. cit.*, Richard Stone "Simple Transaction Models, Information and Computing," *Rev. Econ. Stud.* (1951-52), XIX (2), 67-84 and John S. Chipman, *op. cit.* The latter introduces an ingenious way of allowing for lags of different length.

The economics of the Spending → Production lag are excellently discussed by Gardner Ackley in section 2 of his article "The Multiplier Time Period," *Am. Econ. Rev.* (June 1951), XLI, 350-68.

have twenty Goods Gaps and twenty kinds of possible unintended Investment, but it would still be true that the latter arises from mutual inconsistency of plans and expectations concerning the production, sale and purchase of goods.

In conclusion, it is worth examining the belief that the income-generation period is equal to the reciprocal of the velocity of circulation of active money.[40] This belief is not true, whether the lags composing the income-generation period be simple lags or weighted averages, for the following reasons:

1. The plan-revision period of Firms and Households need not equal the length of time for which they hold active money balances. For example if a Household receives its income at the beginning of a period and disburses it evenly through the period, it holds money on the average for only half a period.

2. Money received on current account and used to finance Imports or security purchase will later be used to finance Exports or Investment. The speed of this will affect income-velocity but not the income-generation period.

3. Increased dealings in the capital market on capital account will increase active balances (or raise their speed of turnover) without affecting the income-generation period.[41]

4. If there is trade credit so that payment follows purchase "the speed of the flow of the new money is seen to depend on the rate at which an added flow of new goods is forthcoming, and there exists no logical reason for supposing that the rate at which production increases will bear any connection with any pre-existing speed of money flows."[42] Thus the marginal income-generation period may be very different from the average and any calculable income velocity is relevant, if at all, only to the latter.

[40] Fritz Machlup maintained this in his paper "Period Analysis and Multiplier Theory," American Economic Association, *Readings in Business Cycle Theory* (Philadelphia, 1944) where he defined this reciprocal as "the period in which total incomes are equal to the total of active balances" this being "the time which it takes the money in active circulation to complete a circuit flow from income recipient to income recipient" (p. 208). This is "Income Period E." He made a number of reservations, however, some of which are elaborated below.

Some of the following points were made earlier in my article "The Multiplier," *Economica* (Nov. 1948), XV, 267.

[41] Some authors beg these last two points by explicitly ignoring "the financial circulation" which they fail to define adequately.

Machlup states that "An increase in transactions arising from the transfer of assets might lengthen the income period; an increase in the use of money substitutes and of clearing arrangements might shorten the income period. The effects on the propagation speed of the new income flow may be considerable (*op. cit.*, p. 215). This simply assumes equality of the two periods.

[42] Ackley, *op. cit.*, p. 352.

COMPARABILITY OF LABOR CAPACITIES OF FARM AND NONFARM LABOR*

By D. GALE JOHNSON

> When watching the action of demand and supply with regard to a material commodity, we are constantly met by the difficulty that two things which are being sold under the same name in the same market are really not of the same quality and not of the same value to the purchasers. Or, if the things are really alike, they may be sold even in the face of the keenest competition at prices which are nominally different, because the conditions of sale are not the same. . . . But difficulties of this kind are much greater in the case of labour than of material commodities: the true price that is paid for labour often differs widely, and in ways that are not easily traced, from that which is nominally paid.[1]

In the above paragraph and his later discussion, Marshall indicates that wage rates or money earnings, by themselves, provide an inadequate picture of the functioning of the labor market because the laborers being compared are not of equal efficiency or the conditions of sale of labor are dissimilar. Any analysis of the functioning of the farm labor market is beset by difficulties of both kinds. This is especially true if one wishes to determine whether the labor market has tended to equalize the returns to farm and nonfarm labor. In this paper only the first of the two main problems is considered—the general question of the efficiency of the farm work force compared to the nonfarm work force.

Because the word "efficiency" has so many different connotations, the term "labor capacity" is used in its place. Our measure of the labor capacity of the farm labor force is the job distribution that a random sample of the farm labor force would have in nonfarm employment if each worker held the job which for him had the greatest net advantage. This measure is subject to two restrictions: it relates to a given time

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[1] Alfred Marshall, *Principles of Economics* (London, 1936) 8th ed., pp. 546-47.

and it assumes that farm migrants find jobs in competition with the nonfarm labor force, as it actually exists at the time, in a given labor market. The measure of relative labor capacity that can be determined is ratio of the average money earnings reflected by the (hypothetical) nonfarm job distribution of farm labor force to the average money earnings of the nonfarm work force. The measure assumes that farm people have the same set of attitudes concerning the nonmonetary advantages and disadvantages of nonfarm jobs as do nonfarm people.[2]

This study attempts to determine the relative labor capacity of farm people by analysis of the job experience of farm persons who have migrated to nonfarm areas. The materials have come largely from the 1940 Census of Population and cover persons who migrated from farms between January 1, 1935 and the time the 1940 Census was taken.[3] The general approach has been to estimate the money earnings reflected by the farm-nonfarm migrant population, standardized to eliminate the effects of age, sex and color differences relative to the parent farm population, and to compare this estimate to the labor earnings of the nonfarm population. The conclusions reached later are based upon two significant hypotheses: (1) The migrants from farm areas are representative of the parent population in all significant characteristics, excepting age, sex and color. (2) The actual job experience of farm migrants in nonfarm areas is indicative of the labor capacity of farm migrants. This implies that recent migrants from farms are able to find the jobs for which they are best suited under given labor market conditions.

It has not been possible to find evidence that would support or reject the second hypothesis. It seems probable that the first job held by many migrants is not the one best suited to his particular labor capacities. The average time the farm migrants had been in nonfarm communities was approximately two and one-half years. Consequently, the majority of them had had an opportunity to search for different jobs, though an important fraction had not. It is quite possible therefore that the dependence of the analysis upon this hypothesis has had the effect of a slight underestimation of the labor capacity of farm people.

[2] Reference is made only to the labor capacity of farm people as evidenced by the nonfarm jobs they might hold and not also to the farm jobs nonfarm people might hold. This has been done because the net transfer of labor in the United States has been from farm to nonfarm and the relevant comparison is the one chosen.

[3] A migrant was defined as a person who lived in 1935 in a county or city of 100,000 or over, different from the one in which he lived at the time the 1940 Census was taken. Thus not all persons who changed from a farm to a nonfarm residence between 1935 and 1940 were counted as migrants if the move was within a county and not to a city larger than 100,000.

I. *Selectivity of Rural Migration*

Migration from farm to nonfarm communities is a selective process with respect to certain population characteristics. The migrants included in the present study were younger than the parent population.[4] The migrant stream included slightly fewer males[5] and significantly fewer nonwhites than the rural farm population of 1940.[6]

Selectivity of migration with respect to age, sex and color is of little significance in the present instance. The effects of these types of selectivity can be reflected in our estimates of labor capacity. Migration, however, may be selective with respect to other characteristics that are related to labor capacity, such as intelligence, manual dexterity and education. The hypothesis that the farm migrants to nonfarm areas represent a random sample of the parent farm population with respect to characteristics other than age, sex and color is not rejected by three different types of evidence that bear on the issue.

1. The median number of years of school completed by farm migrants 25 to 34 years old was almost identical to that of the farm population of the same age—about 8 years of schooling. About 11.5 per cent of the migrants had some college education compared to 8.8 of the rural farm population. Almost the same proportion graduated from college (4.3 and 3.8 per cent). It does not appear that the farm migrants were appreciably better educated than the farm nonmigrants.[7]

2. If there is a positive relation between the labor capacity of farm migrants and the level of farm income in their original community, the distribution of farm migrants by the region of origin does not indicate that the farm migrants came predominantly from either the high or low farm income regions. An analysis of the data on net off-farm migration rates by states for the decade of the 'thirties as a whole revealed a weighted correlation of -0.38 between the level of net farm income per worker and the rate of migration from farms. The correlation coefficient, which is just significant at the one per cent level, indicates that the low income states had a somewhat higher rate of net off-farm

[4] The white farm migrants to nonfarm areas had a median age about 10 years lower than the white farm nonmigrants if only persons over 14 are included. The nonwhite median for migrants was about 4 years lower than for the nonwhite farm nonmigrants. See Sixteenth Census of the U.S., 1940, *Population, Internal Migration, 1935 to 1940, Age of Migrants*, Table 9.

[5] See Sixteenth Census, *Population, Internal Migration, Social Characteristics of Migrants*, Table 2.

[6] See Sixteenth Census, *Population, Internal Migration, Color and Sex of Migrants*, Table 2. The nonwhite farm population was 16.2 per cent of the total farm population in 1940; only 12.2 per cent of the farm migrants to nonfarm communities were nonwhites.

[7] Sixteenth Census, *Population, Internal Migration, Social Characteristics of Migrants*, Table 7.

migration than the higher income states. However, it should be noted that for the particular group of migrants included in our data, the regional distribution of the origin of migrants did not indicate any relationship between the level of farm incomes and the rate of migration.

3. There have been several score of studies made for individual communities in an attempt to determine if migrants represent a selected group from the parent population that is in some way superior or inferior. These studies, made by sociologists, have used various definitions of superiority or inferiority, but most of the research workers had in mind a concept that had a significant relation to economic productivity. In a review and analysis of this literature, Dorothy S. Thomas found four conflicting hypotheses: (1) Cityward migration selected the superior elements of the parent population; (2) Cityward migration selects the inferior elements; (3) Cityward migration selects from the extremes; and (4) Cityward migration represents a random sample of the parent population. She concludes as follows: "We have, then, evidence of a sort that migration selects the better elements, the worse elements, both the better and the worse, and also that it is unselective. Even though we may decide that the evidence cited is tenuous, it is not improbable that selection does operate positively, negatively, and randomly, at different times, depending on a variety of factors that, up to the present, have not been adequately investigated."[8]

Sorokin and Zimmerman arrived at the following conclusions from an analysis of the literature: "There is no valid evidence that migration to the cities is selective in the sense that the cities attract in a much greater proportion those from the country who are better physically, vitally, mentally, morally, or socially, and leave in the country those who are poorer in all these respects [italics omitted]. There is also no evidence that the reverse is true."[9]

Though I have not been able to provide evidence denying the possibility of selectivity in the migration process, it is significant that the hypothesis that no important degree of selectivity exists cannot be rejected on the basis of available data. Consequently, in this paper it is assumed that there is no selectivity.

II. *Occupational Experience of Rural Farm Migrants to Nonfarm Areas*

The published data from the 1940 Census of Population do not provide estimates of the wage and salary incomes of farm migrants to nonfarm areas. The published data indicate the employment status of the

[8] Dorothy S. Thomas, "Selective Migration," *The Milbank Memorial Fund Quart.*, 1938, XVI, 403-07.

[9] Sorokin, P. and Zimmerman, C. C., *Principles of Rural-Urban Sociology* (New York, Henry Holt and Co., 1929), p. 582.

migrants and the occupational category of the employed migrants. On the basis of certain assumptions, which are indicated below, these data are translated into an estimate of annual labor earnings of the migrant group.

Recently the Bureau of the Census tabulated data on the wage and salary incomes of farm migrants to urban areas for samples of migrants from the Corn Belt and from the Cotton Belt. The wage and salary incomes relate to 1939; consequently, some of the migrants were still living and working in farm areas during part or all of 1939. Thus the wage and salary data cannot be used directly but required adjustment to reflect the effect of lower money incomes on farms as well as for the more obvious adjustments for age. In this section, I will present the results obtained from the published data and later show the degree of consistency between the two sets of estimates.

A. *Incidence of Unemployment*

These data, summarized in Table I, indicate that rural farm migrants to urban areas had almost identically the same employment status as the urban population. Of those classified as members of the labor force, 84.4 per cent of the farm migrants were employed while 84.7 per cent of the labor force members resident in urban areas were employed.[10] Some difference does exist in the case of migrants to rural nonfarm areas. Roughly 3 per cent fewer migrants than nonmigrants were employed.[11]

Corroborating data are provided by a Census sample survey of the employment experience of migrants in 1948. These data cover migrations of one-year duration or less. In this case migrants had rather different rates of unemployment than the nonmigrants. However, the male migrants from farm to nonfarm areas had roughly the same proportion of unemployment (9.2 per cent) as the migrants from nonfarm to nonfarm areas (8.1 per cent).[12] These rates of unemployment were much higher than for the total male population, which was 3.6 per cent. The higher rate of unemployment among the migrants was probably due to the relatively short time interval considered.

[10] No difference remains when the employment data are adjusted for the differences in age distribution.

[11] Farm migrants to farm areas had the same employment status as the total farm population.

[12] Bureau of the Census, *Current Population Reports, Labor Force*, Series P-50, No. 10, Table 4. The two figures could have been drawn from the same populations. (See *ibid.*, p. 5.)

TABLE I.—DISTRIBUTION OF RURAL FARM MIGRANTS AND TOTAL POPULATION IN THE LABOR FORCE IN URBAN AND RURAL NONFARM AREAS BY EMPLOYMENT STATUS, 1940, FOR U.S.*
(Per cent)

	Both Sexes			Males		
	Em.[a]	S.W.[a]	E.W.[a]	Em.	S.W.	E.W.
Rural Farm Migrants by Destination						
Urban	84.4	11.2	4.4	81.1	13.4	5.5
Rural Nonfarm	79.6	12.9	7.5	77.9	13.7	8.4
Rural Farm	91.4	5.3	3.3	91.7	4.9	3.4
Total Population						
Urban	84.7	11.0	4.3	81.4	13.8	4.8
Rural Nonfarm	82.2	10.6	7.2	81.0	11.1	7.9
Rural Farm	91.7	4.4	3.9	92.1	4.0	3.9

[a] Em = Employed; S.W. = Seeking Work; and E.W. = Emergency Work.
* Source: Sixteenth Census, *Population, Internal Migration, 1935 to 1940, Economic Characteristics of Migrants*, Tables 2 and 8. Data relate to individuals 14 years or older.

B. *Occupational Distribution of Employed Migrants*

The previous section indicated that farm migrants to nonfarm areas found employment as readily as nonmigrants, if a certain time for adjustment was allowed. The question to which we now turn is the nature of the jobs which the farm migrants obtained.

Table II compares the occupational distribution for employed male and female migrants to urban and rural nonfarm areas with the occupational distribution of the nonmigrants. In the table, the occupational groups are ranked according to the medium wage or salary income of male workers employed for 12 months in 1939.[13]

It will be noted that the farm migrants to urban areas are not as fully represented in the higher income groups as the urban nonmigrants are. In the top four groups, which contain 56.4 per cent of the nonmigrants, are found only 38.0 per cent of the farm migrants and much the same situation prevails in the rural nonfarm areas—45.0 per cent of the nonmigrants fall in the top four groups while only 28.1 per cent of the farm migrants do. Comparison of female farm migrants and nonmigrants shows much the same thing.

The importance of the differences in the occupational distribution of the migrants and nonmigrants depends upon the differences in the wages in the various occupational groups. If the wage differences were substantial, one would expect the occupational distribution of the farm

[13] There are only minor differences between the ranks of wages and salaries for males and females. (See Table III.)

TABLE II.—PERCENTAGE DISTRIBUTION AMONG MAJOR OCCUPATION GROUPS OF EMPLOYED FARM MIGRANTS AND NONMIGRANTS, BY SEX, IN URBAN AND RURAL NONFARM AREAS IN 1940

	Male		Female	
	Non-migrants (per cent)	Farm Migrants (per cent)	Non-migrants (per cent)	Farm Migrants (per cent)
Urban				
Proprietors, Managers and Officials	12.4	5.4	3.7	1.4
Professional and Semiprofessional	6.5	3.9	11.2	13.7
Clerical and Sales	18.4	12.7	33.5	16.8
Craftsmen and Foremen	19.1	16.0	1.2	0.4
Operatives	22.8	26.4	21.2	13.0
Service Workers	8.9	12.7	11.6	17.1
Laborers	10.5	16.9	1.9	1.4
Domestic Service	0.5	0.8	5.4	35.5
Farmers and Farm Managers	0.6	3.9	0.1	0.1
Farm Laborers and Foremen	0.4	1.1	0.1	0.6
Total	100.1	99.8	99.9	100.0
Rural Nonfarm				
Proprietors, Managers and Officials	12.8	6.9	6.1	3.1
Professional and Semiprofessional	4.7	4.0	13.2	18.9
Clerical and Sales	9.9	5.1	21.8	9.7
Craftsmen and Foremen	17.6	12.1	0.8	0.3
Operatives	25.6	22.3	20.1	13.2
Service Workers	5.1	14.3	11.2	17.0
Laborers	15.3	18.1	2.9	2.2
Domestic Service	0.6	0.6	22.4	32.7
Farmers and Farm Managers	2.4	2.1	0.3	0.1
Farm Laborers and Foremen	6.0	14.2	1.3	2.9
Total	100.0	99.6	100.1	100.1

* Source: Sixteenth Census, *Population, Internal Migration, Economic Characteristics of Migrants*, Table 10.

migrants to reflect substantially lower income than the occupational distribution of nonmigrants. One way of determining the extent of the difference is to weight the occupational distribution by the median incomes actually received in each occupation. The census provides information on the money income from the receipt of wages and salaries. Any nonmoney income such as the perquisites to farm laborers and domestic servants is not given. The income of self-employed persons is also excluded. Likewise data are available only for the United States as a whole and not for urban and rural nonfarm areas. Of the three main limitations of the data, only the first—failure to include nonmoney wage or salary income—is important. Since relatively few urban work-

TABLE III.—INCOME REFLECTED BY OCCUPATIONAL DISTRIBUTION OF EMPLOYED FARM MIGRANTS AND NONMIGRANTS IN URBAN AND RURAL NONFARM AREAS, BY SEX, 1939

Residence in 1940 and Sex	Farm Migrants (dollars per year)	Nonmigrants (dollars per year)	Per Cent Migrants of Nonmigrants
Urban			
Male	1075	1260	85
Female	560	700	83
Rural Nonfarm			
Male	960	1160	83
Female	590	660	89

Based on data in Table II and data below.
Median annual wage and salary incomes by occupational groups were as follows:

	Male	*Female*
Proprietors, Managers, and Officials	$2,136	$1,107
Professional and Semiprofessional	1,809	1,023
Clerical and Sales	1,359	883
Craftsmen and Foremen	1,309	827
Operatives	1,007	582
Service, except Domestic	833	491
Laborers, except Farm	673	538
Domestic Service	429	296
Farmers and Farm Managers	373	348
Farm Laborers	309	76

* Source: Sixteenth Census, *Population*, Vol. III, *Labor Force*, Part I, Table 72. In calculating medians, incomes of less than $100 were not included to exclude most unpaid family workers. Comparisons do not entirely eliminate effects of age.

ers fall in the self-employed category, failure to include income from that source is unimportant.[14]

The results of these calculations are presented in Table III. They show that the income reflected by the occupational distribution of the rural farm migrants, had they received the median income of each occupational group, would have been somewhat less than the income reflected by the occupational distribution of urban and rural nonfarm nonmigrants. The differences are of the magnitude of 11 to 17 per cent less.

Are these differences due solely to the different innate capacities of the groups being compared? One obvious source of difference is age.

[14] Out of a total of 40,450,000 male workers in 1948, something less than 4,000,000 were self-employed in pursuits other than agriculture. The earnings of the self-employed professional workers in 1947 who were about one-third as numerous as the salaried professional workers, were about 40 per cent greater than the income of salaried professionals. However, most of the self-employed were in the proprietors, managers, and officials group and the self-employed earned about a sixth less than the salaried. See Bureau of the Census, *Current Population Reports*, Series P-60, No. 5, Table 17.

The farm migrants are younger than the nonmigrants in the areas to which the farm migrant went. Farm migrants to nonfarm areas fall into age brackets that earn less than the average of either the parent or the absorbing population.

The calculations given in Table III eliminate the effect of age upon the level of earnings within an occupational group. An adjustment of the data to include the effects of age upon the occupational distribution of a population can be made by comparing the occupational distribution of nonfarm people of the same age distribution as the farm migrants with the actual occupational distribution of nonfarm people.

Such an analysis cannot be made directly from the data at hand. Census data are available showing the occupational distribution by age only for the country as a whole and not for nonfarm groups. This distribution was adjusted by omitting farmers and farm managers and farm laborers. The Census data gave the age distribution of farm persons migrating to urban areas, but not all such migrants were in the labor force. It was assumed, since the proportion of the migrants that was in the labor force was roughly the same as for urban nonmigrants, that for any age group labor force participation was the same for rural farm migrants and urban nonmigrants.

When these adjustments were made, it indicated that the income reflected by the actual age distribution of the male farm migrants was $1,230 while the male urban nonmigrants, excluding farmers and farm laborers, had an average of $1,270.[15] This would indicate a difference of roughly $40 due to occupational differences associated with the age distribution of farm migrants. If this adjustment is applied to the data in Table III, it would mean that the male farm migrants to urban areas would earn on the average about 11 per cent less than male urban nonmigrants. This would be an income level roughly equal to the average incomes of (1) operatives and (2) craftsmen and foremen.

All of the above calculations on wage earnings relate, of course, to the given set of relative wage incomes by occupational groups. A different set of relative wages combined with either the same or different occupational distribution of migrants and nonmigrants would result in a different comparison of labor earning possibilities.

Data on wage or salary incomes by occupational groups comparable to the data used in the previous analysis are available for 1950.[16] A comparison of the changes in wage or salary incomes by occupational groups indicates that the groups in which farm migrants are concen-

[15] See Sixteenth Census, *Population*, Vol. III, *Labor Force*, Part 1, Table 65, for occupational distribution by age.

[16] See Bureau of the Census, *Current Population Reports, Consumer Incomes*, Series P-60, No. 9, Table 25.

trated to a greater degree than the urban population had the greatest relative rise in earnings. This change in relative wage rates had the effect of reducing the differential between the earnings value (before adjustment for the effect of age on occupational distribution) of male urban nonmigrant and the farm migrants occupational distribution from 15 per cent to 10 per cent. After adjustment for the effects of age, the earnings of male farm migrants were about 8 per cent less than the male urban nonmigrant.[17]

The conclusion that one might draw from this analysis is that labor employed in agriculture, on the average, has a labor income capacity of roughly 90 per cent of the labor income capacity of our urban and rural nonfarm populations, for similar age and sex distributions.

This estimate is for the nation as a whole. The migrants from different regions may have fared rather differently. The next section analyzes the experience of migrants by region of origin.

TABLE IV.—INCOME REFLECTED BY OCCUPATIONAL DISTRIBUTION OF FARM MIGRANTS TO URBAN AND RURAL NONFARM AREAS CLASSIFIED BY REGION OF ORIGIN AND SEX, 1939*

Residence in 1940 and Sex	Region of Origin			
	Northeast	North Central	South	West
Urban				
Male	1125	1120	1030	1280
Female	620	590	555	650
Rural Nonfarm				
Male	960	1035	910	980
Female	575	640	545	670

* Source and method of calculation: see Table III. The income reflected by the occupational distribution of nonmigrants for urban areas was $1,260 for males and $700 for females and for rural nonfarm areas was $1,160 for males and $660 for females.

III. *Occupational Experience of Farm Migrants from Different Regions*

The farm migrants from different regions to nonfarm areas did not achieve the same occupational distributions. Table IV shows the earning value of the occupational distributions of the migrants from the four regions to urban and rural nonfarm areas. The West farm migrants to urban areas had the highest earning value, while the South farm migrants had the lowest when the migrants went to urban areas. The North Central farm migrants did the best of migrants to rural nonfarm areas while the South farm migrants did the least well.

[17] Calculations for migrants to rural nonfarm areas indicate a difference of 10 per cent at the 1950 relative wage levels.

The relatively poor showing of the South seems to be due to the color composition of the South migrants. The published Census data did not give separate data for the occupational distribution of whites and nonwhites. However, a special tabulation of data from the 1940 Census of Population permits a comparison of the occupational distribution of male white farm migrants to urban areas for the Cotton Belt and the Corn Belt.[18] A calculation of the income value reflected by the occupational distributions was the same as for the Corn Belt.[19]

Consequently, if the occupational distributions accurately reflect the capacity of the farm migrants and thus of the farm population, it may be concluded that the labor capacity of the white farm labor force in the South is approximately the same as in the rest of the nation. This conclusion is somewhat surprising in light of the significant differences that exist between agricultural labor earnings in the South and in the rest of the nation.[20] The writer considered the possibility that the farm migrants in the South came in disproportionate numbers from the rural areas with the highest farm incomes. This might then explain the relative ease with which Southern farm people fit into the nonfarm occupation pattern.

The hypothesis does not appear to be valid, however. Though data are not available to show the origin of white farm migrants by small

[18] For a description of the sample, see Section IV below.

[19] Though the Cotton Belt does not include all of the South and the Corn Belt does not include all of the North Central States, the income value reflected by the farm migrants in the Cotton Belt farm (both white and nonwhite) migrants differed from that of the South as a whole by less than one per cent, while the difference between the Corn Belt and the North Central States was about three per cent. This indicates that the samples were quite representative of the larger areas. These calculations and those referred to in the text assume that the rates of pay in the same occupational groups were identical in all regions. The validity and significance of this assumption is discussed in Section IV.

[20] For the years 1940 and 1945, the writer has estimated that annual labor returns to farm workers were as follows (in dollars):

Region	*1940*	*1945*
United States	$385	$1,135
New England	505	1,090
Middle Atlantic	565	1,105
E. N. Central	505	1,455
W. N. Central	500	1,745
S. Atlantic	260	710
E. S. Central	260	540
W. S. Central	315	760
Mountain	505	1,560
Pacific	530	2,202

These data relate to all labor in the South. Adjustments based on the relationship between the value of products sold, traded or used on farms of white farm operators and all farm operators in the South, result in estimates of labor income for white farm labor 10 to 12 per cent higher than those given above. Since some Negro labor is used on farms operated by white operators, there is a small, but undertermined, downward bias in the estimates of white farm labor income in the South.

areas, such as counties, data on all farm migrants indicate that the rate of migration is as high, if not higher, from counties with the lowest levels of living as it is from counties with the highest levels of living.[21]

IV. *Wage or Salary Income of Farm Migrants to Urban Areas*

Because wage and salary data were not available for all farm to urban migrants, it was necessary to derive an estimated level of earnings from their occupational distribution. This estimate was based on the assumption that the median earnings of the migrant were the same as the median earnings for the occupational group involved. Through the cooperation of the Bureau of the Census, it has been possible to obtain the wage and salary data for 9,500 migrants from the Corn Belt, 8,900 white and 4,000 nonwhite migrants from the Cotton Belt to urban areas.[22]

The wage or salary data for the sample of migrants cannot be directly compared with the data provided in the Census for any urban group. First, the wage or salary income is for the year 1939 and at least 24 per cent of the individuals included in the sample resided on farms part or all of 1939. Thus, the income data do not indicate directly the income of the farm migrants in urban occupations. Most of the individuals who lived on farms some part of 1939 either would not have had any wage or salary income while living on farms because they were farm operators or unpaid family workers or would have worked as farm laborers at a lower level of money income than the average urban wage or salary level. Second, the age distribution of migrants is substantially different from that of urban nonmigrants. Despite these two difficulties, the availability of these data provide an opportunity to check the general validity of the assumptions involved in the earlier analysis.

The migration period covered in the Census questionnaire was from April 1, 1935 to the time the Census enumeration was actually made,

[21] The countries in the 16 states of the South were divided into quartiles according to the rural farm level of living index developed by Hagood. The average (unweighted) migration rates for the counties in each quartile were calculated for 1930-40 decade. The quartiles had the following average rural level of living indexes: 1st, 108; 2nd, 85; 3rd, 74; and 4th, 60.6. The average migration rates were: 1st, minus 13.1; 2nd, minus 13.1; 3rd, minus 15.4; and 4th, minus 13.2. Source of data: as to levels of living: Margaret Jarman Hagood, "Rural Level of Living Indexes for Counties of the United States, 1940" (Washington, 1943); and as to migration: Eleanor H. Bernert, "County Variation in Net Migration from the Rural Farm Population, 1930-40" (Washington, 1944).

[22] These data were obtained jointly by the Scripps Foundation for Research in Population Problems of Miami University, the Bureau of Agricultural Economics of the United States Department of Agriculture, and Agricultural Economics Research of the University of Chicago. Mr. and Mrs. Donald J. Bogue contributed substantial amounts of their time and energy to the study. It should be noted that the sample included only migrants that remained within the same state.

which was subsequent to April 1, 1940. Thus persons who migrated within an approximate 15-month period following January 1, 1939 would have spent some time on farms during 1939. On the basis of Bureau of Agricultural Economics estimates of departures from farm to nonfarm areas, and assuming that there was no greater memory bias for migrants leaving farms from 1935 through 1938 compared to migrants after that date, 24 per cent of the migrants lived part or all of the year 1939 on farms. The wage or salary income of male farm residents in 1939 was substantially below that of urban residents. In fact, for employed male farm residents without other income, but with $1 or more of income, it was $369; for the employed male urban resident, the median was $1,188.[23] Further, 30.5 per cent of the employed male farm residents without other income had no money income and presumably were unpaid family workers, while only 5.6 per cent of the urban group fell into this category. It is reasonable to assume that the male farm migrant would not have had money wage or salary earnings during the period of residence on farms in 1939 greater than 30 per cent of the amount he did or would have earned in an urban area. Consequently, I have assumed that the median earnings of the farm to urban migrants should be increased by a minimum of 10 per cent to adjust for the factor discussed here.[24]

The adjustment for the difference in age distribution was made by estimating the level of wage or salary income employed urban residents would have had if their age distribution had been the same as that of the farm migrants. This estimate was then subtracted from the earnings level reflected by the actual age distribution of the employed urban residents. The results of these adjustments are indicated in Table V.

Table V also provides a comparison between the adjusted wage or

[23] Sixteenth Census, *Population, The Labor Force (Sample Statistics)*, Wage or Salary Income in 1939, Table 10a.

[24] The method of arriving at this estimate may be indicated briefly. As noted in the text, 24 per cent of the migrants lived on farms part or all of 1939. If the movement occurred at the same rate during the 15 months through March, 1940, a fifth of this group of migrants would have lived all of 1939 on farms, while of the remaining four-fifths their average experience would have been a half year of work on farms and a half year of work (or seeking work) in urban areas. Thus taking the urban wage as one and the farm resident's salary or wage income as three-tenths, the following weighted average for all migrants is obtained:

$$
\begin{aligned}
0.76 \times 1.00 + &= 0.760 \\
0.048 \times 0.30 \quad &= 0.014 \\
0.192 \times 0.65 \quad &= 0.124 \\
\hline
&\quad 0.898
\end{aligned}
$$

This average indicates that the actual wage or salary income of the migrant should be increased by about 11 per cent to adjust for the inclusion of income earned while a farm resident.

TABLE V.—MEDIAN ANNUAL WAGE OR SALARY INCOME OF EMPLOYED MALE RURAL FARM MIGRANTS TO URBAN AREAS AND OF EMPLOYED MALE URBAN RESIDENTS, AND ADJUSTMENTS FOR RESIDENCE AND AGE, CORN BELT AND COTTON BELT, 1939

Area and Color	Migrants' Median Wage or Salary Income[a] (dollars)	Residence Adjustment[b] (dollars)	Age Adjustment[c]	Migrants' Adjusted Income (dollars)	Urban Median Wage or Salary Income[d] (dollars)	Income Ratio: Migrants to Urban
Corn Belt, White	954	95	126	1,175	1,218	0.96
Cotton Belt						
South, White	690	69	122	881	1,010	0.87
North, White	708	71	134	913	1,040	0.88
South, Nonwhite	412	41	30	483	465	1.04
North, Nonwhite	404	40	50	494	465	1.06

[a] From special tabulations of Census data. Includes all employed workers on private or nonemergency government work in late March 1940, including those with no wage or salary income, but excludes any worker who had $50 or more of income from sources other than wages or salaries. The two standard deviation confidence limits for the sample medians, given in the same order as in the table are: 940–968, 670–710, 693–723, 394–430 and 386–422.

[b] An adjustment of 10 per cent applied.

[c] The age adjustment reflects the difference between the wage or salary level of the distribution of the employed migrants and of the employed urban residents evaluated at the level of earnings by age for the urban residents. For the white migrants, the age distribution and earnings levels by age were those for employed male urban residents for the United States as a whole. Source: Sixteenth Census, *Population, The Labor Force (Sample Statistics), Wage or Salary Income in 1939*, Table 6. For the nonwhite data from Sixteenth Census, *Population, Education, Educational Attainment by Economic Characteristics and Marital Status*, Tables 20 and 35, were used after minor adjustment.

[d] For wage or salary workers without other income on private and nonemergency government work. Median calculated for employed persons including those with no wage or salary income in 1939. For Corn Belt the income figure used is for the North Central States. (Sixteenth Census, *Wage or Salary Income in 1939*, Table 5). For the Cotton Belt, nonwhite, the South as defined by the Census is used (*ibid.*, Table I). The median wage and salary incomes for the two Cotton Belt areas were not available from the Census and it was necessary to estimate the medians. The median of $1,050 for white male employees for the South as a whole was reduced, in each of the two cases, by the percentage that the median family income for white wage or salary workers' families, in the states included in the Cotton Belt area, was below the similar median for the South as a whole. (See Sixteenth Census, *Population and Housing, Families, General Characteristics*, Tables 34 and 45.)

salary medians for the employed male farm migrants and the medians for urban employed workers. The results are, I believe, quite consistent with the estimate of a 10 per cent difference derived for the United States as a whole on the basis of the occupational distributions. Some of the variation in the ratios of the incomes are undoubtedly due to sampling variation. For example, the occupational distribution of the sample of Corn Belt migrants reflected an earnings value of about 3 per cent more than for all the farm to urban migrants from the North Central States. However, the combined occupational distribution for

all Cotton Belt migrants was virtually identical with that for the South as a whole.

The results are, I believe, reasonably consistent with the estimate of a 10 per cent difference in the labor capacities of the farm and nonfarm labor forces for the United States. The high relative incomes of nonwhite migrants in the Cotton Belt offset the somewhat lower relative incomes for whites in the same region. These data support one of the assumptions underlying the analysis of the occupational distributions, namely, that the farm migrants in any occupational group had the same median level of earnings as the urban resident in the same occupational group (after adjustment for age and sex). If this assumption were not valid, the relative money incomes derived from wage data would have been substantially below 90 per cent since the occupational distributions in our area samples were very similar to those for the regions that included the samples.

The data on money wages or salaries seem inconsistent with our earlier results at one point, namely, that the white labor forces in the Corn Belt and the Cotton Belt had the same labor capacities. This conclusion was based on the comparison of the occupational distribution, assuming that the levels of earnings by occupational groups were the same in the two areas. The data given in Table V on urban wage or salary incomes indicate that this assumption is not valid since the occupational distribution of nonfarm white workers in the South is nearly the same as for white workers in the nation as a whole.[25] Does the difference in the earnings of the urban residents in the South and in the North Central States reflect differences in the labor capacities of the nonfarm labor forces of the two regions? Though I cannot justify my conclusion in the space available to me, I would answer the question in the negative. Some of the difference can be explained in terms of the distribution of workers by size of community and size of plants in which workers are employed. Much of the remainder was due to the demand and supply relations in the labor market in the South compared to the rest of the nation. There is some evidence that there was a fairly substantial reduction in the labor-earnings differential between the South and the rest of the country from the late 'thirties to the late 'forties.[26]

Even if one accepts the assumption that the difference in labor earnings of urban residents does not reflect any difference in labor capacities,

[25] See D. Gale Johnson, "Some Effects of Region, Community Size, Color and Occupation on Family and Individual Income," *Studies in Income and Wealth*, Vol. XV (New York, National Bureau of Economic Research, 1952), p. 64. See also pp. 57-63 for a discussion of wages and labor earnings in the South compared to other regions.

[26] *Ibid.*, pp. 57-63.

there is a difference in the ratio of farm migrant and urban incomes in the Corn Belt and Cotton Belt.

It should be noted, however, that the occupational distribution of the Corn Belt migrants reflected an earnings value of about 3 per cent more than for all the farm to urban migrants from the North Central States, while the combined occupational distribution for all Cotton Belt migrants was virtually identical with that for the South as a whole. Thus if one reduced the ratio of migrant to urban incomes from 0.96 to 0.93, this might be a reasonable estimate for the North Central States as a whole. While the difference that remains of approximately five per cent in relative incomes after this adjustment is probably greater than can be explained by sampling errors, it does not seem unduly large given the other adjustments that have been made in the data.

The major surprise in the results is the nonwhite results. The consistency of results for the two samples creates doubt that the high ratio can be explained by sampling variability.

V. *Qualifications and Conclusions*

If the occupational experience and wage or salary income of recent migrants from farm to nonfarm areas can be used as an indication of labor capacity, and if farm migrants are representative of the parent farm population,[27] then farm people have a labor capacity approximately 90 per cent of nonfarm people of the same age and sex. Two main hypotheses are indicated in this summary statement and the conclusion obviously depends upon their acceptance.

I believe that the first of the hypotheses may be subject to some reservation. The occupational and wage experience of farm migrants in the first year or so following migration may underestimate the level of their labor capacities. Such an underestimate would occur if migrants tended to improve their occupational or income situation to a greater degree than the rate of advancement normally associated with age and experience. This proposition is one that can be put to empirical test and such a study is now under way.

It should be noted that in all analyses and calculations medians have been used instead of arithmetic means. However, we know that distributions of wage or salary data are not symmetrical, but exhibit an important degree of skewness, with the median less than the means. If the ratio of the mean to median were the same for all of the wage distributions used, it would make no difference in any of the results obtained in our analysis. However, though there is evidence that the ratio of the

[27] It is argued that the farm migrants are representative of the farm population only after adjustment is made for age and sex.

mean to the median is generally larger in the higher than in the lower-paying occupations, given the diversity within the broad occupational groups used by the Census, there is no systematic relationship between the mean and median as the median wage income rises.[28]

Though some doubt may be cast upon the accuracy of means calculated from open-ended distributions, comparisons were made using the calculated means as well as the medians in estimating the earnings value of occupational distributions. In every case the change in the results was very minor. For example, though the ratio of the income value of the occupational distribution of male farm migrants to urban areas to the same estimate for urban nonmigrants was slightly less when means rather than medians were used, the results when rounded to the nearest per cent were identical. In general the effect of using the estimated means rather than medians would not have changed the results given in Table III by more than one per cent.

The results obtained have several important empirical or practical applications, of which two may be noted quite briefly. One is the relevance of the results to comparisons of farm and nonfarm incomes, or more specifically, to comparisons of labor returns to farm and nonfarm workers.[29] Any comparisons of returns for large and relatively heterogeneous labor groups are always suspect, unless an effort is made to determine the relative equivalence of the labor capacities of the groups. The present results, however, should not be used directly without first standardizing the labor groups being compared for age and sex distributions. If this is done, it may well be found that the farm labor force has a larger proportion of its members in the age groups with lowest earnings. However, the effects of the unfavorable age distribution are likely to be largely offset by the relatively small proportion of females in the farm labor force compared to most other groups. Using 1940 urban wage distributions by age and sex as weights, the 1940 farm labor force had an earning capacity about 4 per cent below the nonfarm labor force. Thus, if real labor returns were as much as 14 per cent lower for the average farm worker than for the average nonfarm worker, this might well be consistent with equal real returns for comparable workers.

[28] Estimates of the mean for the seven nonfarm occupational groups for males were made from the available distributions. The ratios of the estimated mean to medians for experienced workers receiving more than $100 in 1939 were: (1) Proprietors, etc., 1.22; (2) professional and semi-professional, 1.20; (3) clerical and sales, 1.14; (4) craftsmen and foremen, 1.06; (5) operatives, 1.07; (6) service workers (excluding domestic), 1.18, and (7) laborers, 1.12. Source: Sixteenth Census, *Population*, Vol. III, *Labor Force*, Part I, Table 72.

[29] See L. H. Bean, "Are Farmers Getting Too Much?" and comments by D. Gale Johnson and J. D. Black, Rev. Econ. Stat., Aug., 1952, XXXIV, 248-61.

A second application of these results is the assurance that they provide to new employers who might wish to locate in areas of low farm incomes. The evidence indicates that such employers will find individuals having or capable of quickly achieving a wide range of skills. Only in the managerial and professional categories is it likely that new employers will have difficulty in recruiting a labor force roughly equivalent to that available almost anywhere in the United States. The evidence provided by the data for farm migrants is perfectly consistent with the favorable labor productivity experiences of many firms that have located in rural areas where farm incomes have been low.

FUTURES TRADING AND HEDGING

By Holbrook Working*

A good deal of difference of opinion on the utility of futures trading persists even among economists who have studied the subject rather closely. Some, at least, of this disagreement is traceable to imperfect concepts that emerged in connection with early academic studies of futures trading. Such concepts have tended to survive on the strength of their partial validity, despite shortcomings evident to the well-informed. Businessmen and others who are intimately acquainted with futures trading and its consequences tend to realize (often unconsciously) the defects of such imperfect concepts, to employ the concepts so far as they are valid and useful, and to avoid drawing any seriously mistaken conclusions from them. People who have little direct knowledge of futures trading and its observable results have no such protection against false inferences. If, like most economists, they are accustomed to rely on deductions from what seem to be well-established premises, they are especially vulnerable to the imperfections of basic concepts.

I. *Origin and Nature of Futures Trading*

Much of the popular suspicion of futures trading stems from a sense of mystery associated with it. It is in this respect, and some others, rather like bank credit. Futures trading, like banking, is an institution that developed as a contribution to efficiency of a relatively free competitive economy. A primitive form of futures trading emerged spontaneously in various market centers at least as early as 1850. Only in the grain trade at Chicago, however, was the demand for a means of hedging commercial risks then strong and persistent enough to permit this unconventional form of trade to survive the fluctuations in speculative interest, overcome conservative opposition, and live through the stormy period of experimentation necessary to put it on a firm footing. When that had been accomplished at Chicago, the new form of

* The author is associate director of the Food Research Institute, and professor of prices and statistics, Stanford University. This paper has been prepared under a research grant from the Merrill Foundation for Advancement of Financial Knowledge.

trading was soon adopted at other market centers and for other commodities than grains.[1]

Futures trading in commodities may be defined as *trading conducted under special regulations and conventions, more restrictive than those applied to any other class of commodity transactions, which serve primarily to facilitate hedging and speculation by promoting exceptional convenience and economy of the transactions.*

This may seem to some an inadequate definition. It does not say that futures trading is buying and selling for deferred delivery; it draws only a slender line of distinction between futures transactions and "cash" transactions (dealings in the "actual commodity"); and it makes the distinction between futures trading and other sorts of trading turn primarily on *purpose* rather than on more easily and objectively recognizable criteria. All of these characteristics are in fact merits of the definition.

It would be inaccurate to define futures trading as always involving purchase and sale for deferred delivery. Trading in the September wheat future, for example, is done in the month of September as well as in earlier months, and in that month it often happens that some sellers of September wheat intend to make immediate delivery, and the purchaser knows that he may expect to receive immediate delivery. The price of the future is then in fact a spot price. One might, of course, qualify the statement by saying that *most* futures trading is for deferred delivery. The statement would then be true, but objectionable in a definition because it would focus attention on a characteristic (deferred delivery) that has little distinguishing value. A great deal of buying and selling that is *not* futures trading involves delivery at some later time. In international commodity trade in staples, purchases calling for delivery two or three months or more in the future are commonplace, quite apart from true futures trading, and independently of whether or not futures trading exists in the commodity. Much of the trade in manufactured products as diverse as flour, steel rails, and machine tools (none of which has futures trading) involves purchase on contracts entered into several months in advance of the specified delivery date; in the case of machine tools, the interval may sometimes

[1] This, in two sentences, is the story that can be read from scattered comments in Charles H. Taylor, *History of the Board of Trade of the City of Chicago* (Chicago, 1917). Passages in Vol. I, pp. 146-47, 192, 217, 317 and 332, among others, cover the main developments through 1865, when the Board of Trade at last assumed responsibility for aiding and governing the conduct of futures trading.

H. C. Emery, in *Speculation on the Stock and Produce Exchanges of the United States* (New York, 1896) traces the history of trading that had at least some essential characteristics of that done in futures, from institution of the use of warrants by the East India Company in 1733 (p. 35), and says that "Futures were sold in some kinds of grain in Berlin by 1832, and some years earlier in France and Holland" (footnote, p. 41).

be measured in years. Because the people who turn for enlightenment to a definition of futures trading are often unaware of the wide prevalence of forward purchases (except in retail trade), the characterization "usually for deferred delivery" would fail to be generally recognized as only slightly narrowing the area of reference, and would divert attention from more sharply distinguishing characteristics.

The definition given above does in fact distinguish clearly between futures transactions and other transactions—so clearly that there need never be any problem of identification except in such cases as appeared when futures trading was taking its first steps in evolution from other trading and was not yet clearly differentiated. The definition lacks sharpness only in the sense that it does not make futures trading appear very different from other trading. That is a merit, because futures trading in fact has no distinguishing economic characteristics except those stated in the definition, or resulting from them (such as exceptional volume of trading, frequency of transactions, and publicity of quotations).

If a reader feels that the foregoing definition does not distinguish strongly enough between futures trading and other trading in commodities, it may be either because he underestimates the remarkable convenience and economy which are the primary distinguishing characteristics of futures trading, or because he mistakenly believes it to have peculiar characteristics that it does not have. Its extraordinary economy is illustrated by data cited below, in another connection, indicating that a trader in cotton futures could make a very satisfactory net income on the basis of a *gross* profit margin of about 23 cents per thousand dollars worth of transactions—a gross profit of one-fortieth of one per cent.

Mistaken impressions of the difference between futures trading and other trading have been furthered by a language difficulty that arises in connection with the frequent need to speak collectively of all-other-sorts of trading, as against futures trading. There is no good and convenient word for the purpose—and perhaps there cannot be, simply because the need is to designate all of a heterogeneous category except one special, narrow segment of it. "Nonfutures" would be an accurate and transparent term for the purpose, but an awkward one. In this situation, convenience has been served most commonly by using the word "cash" to mean "nonfutures." The practice probably originated in the Chicago grain trade, contemporaneously with the origin of sustained futures trading. Its application involved two shifts of meaning: (1) use of "cash" to designate, not immediate *payment,* as is usual, but immediate *delivery;* and (2) extension of the altered meaning to cover all terms of delivery except those involved in futures contracts. These

changes left the word with no logical merit for the purpose except its brevity. In the cotton trade, the common word for "nonfutures" is "spot." This is inherently more confusing than use of the term "cash," because "spot" continues to be used in the trade also in its specific sense of "immediate delivery"; but the grain trade has lost such potential relative advantage of clarity as it might have had, by using "cash" also as equivalent to "spot" in the sense of "immediate delivery."[2] Most seriously misleading is the frequent resort to use of "actual" to mean "nonfutures," as when purchases on terms other than those of futures contracts are distinguished as purchases of the "actual commodity." That expression is used to *include* forward purchases other than on futures contracts, even though all forward purchases are alike in the fact that there is no acquisition of the actual commodity at the time of purchase.[3] Like the other expressions used for the same purpose, it is a verbal expedient only vaguely defensible in terms of the normal meaning of the expression. Futures contracts involve transactions in the actual commodity as truly as do any other forward transactions.

As regards failure of the definition to give easily and objectively recognizable criteria for identifying futures trading, it should be noted that there is no practical problem of identification except in cases of primitive futures trading, and in such cases purpose is the only available criterion;[4] otherwise, futures trading has always gone under that name, or the equivalent in another language. The definition should indicate the essential distinguishing nature of futures trading, and that is not done by mere listing of superficial technical characteristics, specified in regulations intended to promote convenience and economy. Reliance on these superficial characteristics for definition encounters also the difficulty that they have varied widely from time to time and from place to place. Consequently, definitions based on such characteristics show an historical trend toward increasing complexity and obscurity as later writers tried to remedy technical shortcomings found in earlier definitions.[5]

[2] This is not to say that anybody in either the cotton or the grain trade *is* confused by these practices any more than initiates are confused by the colloquial uses of "buck," "date," and "doll" (all words with the same brevity as "cash" and "spot"); but the usage is a bit frustrating, and even misleading, to an inquiring novice.

[3] Whether use of the expression has its foundation directly in this characteristic, or in the related fact that *speculators* use futures contracts, as they may any forward contracts, to avoid necessity for *handling* of the actual commodity, is a matter of surmise.

[4] The characteristics of convenience and economy being at that stage not well developed.

[5] See, for example, the evolution of definitions from H. C. Emery, *op. cit.*, p. 46, through J. G. Smith, *Organized Produce Markets* (London, 1922), p. 44; C. O. Hardy, *Risk and Risk Bearing* (Chicago, 1923), pp. 205-06; J. B. Baer and O. G. Saxon, *Commodity Exchanges and Futures Trading* (New York, 1949), pp. 132-34.

II. *Hedging as a Basis for Futures Trading*

An interesting conflict of evidence has emerged regarding the comparative rôles of speculation and hedging in sustaining futures trading. Most of the available information prior to about 1920 encourages the view that futures trading rests primarily on an urge for speculation. Hedging is rarely mentioned except in arguments justifying the continuation of futures trading. One gains the impression that hedging, like a hitchhiker, seized the chance for a ride since speculation presented the opportunity. But as statistics have been accumulated that give appropriate quantitative information on futures markets, year in and year out, hedging begins to look like the driver, and speculation in futures like a companion going where hedging gives it opportunity to go.

The first conspicuous evidence in this direction came in studies of the Grain Futures Administration (predecessor of the present Commodity Exchange Authority) that showed the volume of open (outstanding) futures contracts in each commodity rising and falling each year in rough correspondence with the volume of the commodity in commercial hands and likely to be hedged.[6] Speculators tend to be most heavily committed in futures, not during the growing season of a crop, when prices are most variable, but some time after harvest, when large stocks have moved into commercial storage and been hedged.

As between commodities, the volume of open contracts varies likewise with the amount of the commodity that is hedged. The volume of open contracts in wheat futures in the United States during recent years has averaged about 90 million bushels, while the volume in corn futures has averaged not much over 50 million, though corn has been produced in nearly three times the volume of wheat. The reason is that much less corn than wheat gets into commercial hands (farmers rarely or never hedge the stocks that they hold). Oats, produced in volume less than half that of corn and, like corn, stored mainly on farms, has had an average volume of open futures contracts less than half that of corn.[7] So one may go through the list of commodities in which there is futures trading and find, wherever there is information on the amount of hedging use of futures markets, an unmistakable connection between size of the futures market and the amount of hedging that the market is called on to carry.[8]

[6] See, for example, the summary of much earlier work in G. Wright Hoffman, *Grain Prices and the Futures Market*, U. S. Dept. Agric., Tech. Bull. No. 747, January 1941, pp. 33-38.

[7] Data are conveniently available in *Agricultural Statistics* (Washington) for any recent year.

[8] Size of a futures market is better judged by volume of open contracts than by volume of trading because of the wide variation between markets in proportion of trading con-

Though the amount of speculation on a futures market seems to depend so much on the volume of hedging, there is also a connection in the other direction. As between different exchanges dealing in the same commodity, there is a strong tendency for hedgers to prefer to use the exchange which has the largest volume of speculative trading. We shall examine the reason for this later. As regards commodities, it may be observed that in some the volume of hedging has, at times at least, been restricted by absence of sufficient speculative interest to carry the hedges.[9] In the United States, no futures market for a commodity which is chiefly imported, has flourished like the markets for the more important domestically produced commodities. This may be not entirely because the imported commodities give less occasion for hedging, but partly because there are relatively few people in the United States who have acquired an interest in those commodities sufficient to inspire speculation in them.

When one reviews evidence on the earlier history of futures trading, making allowance for the tendency for sporadic news and comment to concentrate on the unusual, and for exceptional outbursts of speculation to draw special attention, one can find reason to think that a desire for hedging opportunities may have always provided the primary support for futures trading. It seems reasonable to suppose that a primitive form of futures trading in grains was able to survive and develop to maturity in Chicago in the middle of the last century, whereas similar trading tried somewhat earlier in Europe was abandoned, because there was much more occasion for hedging the large stocks of grain that came into commercial hands in the Chicago area than for hedging the much smaller stocks of European markets. It seems quite clear that the first successful futures trading in wheat in Great Britain was based on contracts for Californian wheat because that was the wheat which importers found most need to hedge, on account of the long periods over which importers held ownership while the wheat travelled by sailing vessel around Cape Horn to Britain.[10]

One can imagine existence of futures trading purely on the basis

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tributed by "scalping" and by other trading that involves holding a commitment for only a few minutes or hours, with correspondingly small speculative risk and small economic significance.

[9] For example, such an inference seems to follow from information in Blair Stewart, *Trading in Wool Top Futures,* U. S. Dept. Agric., Circ. No. 604, August 1941, pp. 16-26.

[10] See Holbrook Working and Sidney Hoos, "Wheat Futures Prices and Trading at Liverpool since 1886," *Wheat Studies of the Food Research Institute* (Nov. 1938), XV, 125, 142-44. I would now attach less importance than is done there to the uniformity of the quality of the Californian wheat, judging that factor to have been important mainly in the preference for the Californian over the Indian wheat contracts, in which also there was effort for a time to maintain futures trading.

of desire of people to speculate; but apparently futures trading cannot long persist except on the basis of conditions that create speculative risks which somebody must carry, and which some people are led to transfer to others by hedging. The reasons for choosing thus to transfer risks deserve our attention next.

III. *Misapprehensions about Hedging*

It is common to suppose that hedgers exercise no part in determining the price of the commodity in which they deal, and this supposition is substantially valid as regards those who practice hedging uniformly.[11] But most hedgers are engaged in a business that requires them to keep informed on many aspects of the commodity situation, with the result that many hedgers often form quite definite opinions on price prospects. Except in firms that have a strict rule against taking hedgable risks, it is common, therefore, for stocks to be carried unhedged at times, when the responsible individual expects a price advance, and for stocks of the commodity to be hedged at other times. Some individuals and firms hedge stocks only when they are particularly fearful of price decline.

Such discretionary hedging, involving a firm in the practice of both hedging and speculation, seems to be especially prevalent among dealers and processors who handle commodities such as wool and coffee, that have relatively little public speculation in their futures markets.[12] When hedge selling in such a futures market becomes heavy, the price may readily be depressed to a point where a good many dealers and processors are attracted by the possibilities of profit through speculative holding of the commodity. Even among handlers of commodities which attract broad public participation to their futures markets, such as wheat, discretionary hedging is not uncommon.[13] Consequently the existence of futures trading in a commodity and widespread use of futures for hedging do not in fact mean that the responsibilities of price formation are shifted entirely, or even mainly, to people who deal only in the commodity futures.

A major source of mistaken notions of hedging is the conventional practice of illustrating hedging with a hypothetical example in which

[11] Not entirely valid because hedgers are the active agents in determining the *relation* of spot to futures prices, and to that extent they play a major rôle in formation of the spot price.

[12] The case of wool has been documented (*cf.* Blair Stewart, *op. cit.*); the inference that similar situations exist in certain other commodities is based on fairly reliable trade reports.

[13] *Cf.* the Federal Trade Commission's *Report on the Grain Trade* (Washington), Vol. I (1920), pp. 213-27; and Vol. VII (1926), pp. 38-57; and Holbrook Working, "Financial Results of Speculative Holding of Wheat," *Wheat Studies* (July 1931), VII, 417-28.

the price of the future bought or sold as a hedge is supposed to rise or fall by the same amount that the spot price rises or falls. Let us instead consider hedging realistically in terms of some actual prices. The prices to be used will be those for wheat at Kansas City on the first trading day of each month in which futures matured during the crop-year 1951-52.[14]

On the first business day of July 1951, a merchant or processor[15] considering the purchase of the cheapest quality No. 2 Hard Winter wheat (the quality represented by quotations on Kansas City wheat futures) found such spot wheat selling at 3 cents per bushel under the price of the September future. If he bought spot wheat, hedged it in the September future, and carried the wheat until the first business day of September, the results, in cents per bushel, would have been as shown below:

Quotation	Date and Price July 2	Sept. 4	Gain or Loss
Spot No. 2 Hard (low)	229¼	232½	......
September future	232¼	233½	......
Spot premium	−3	−1	+2 (gain)

The profit of 2 cents per bushel is calculated above, in what may seem an awkward way, from the change in spot premium (a negative premium, or discount, on each of these dates). It is awkward, however, only for those to whom it is unfamiliar. The hedger tends to calculate his profits in this way because he would buy the wheat on July 2 primarily for the reason that he could get it at discount of 3 cents per bushel under the price of the September future. In fact, the bargaining which preceded the purchase would normally proceed in terms of discount rather than of price, the price being ascertained by reference to the latest futures price quotation, after sale at a mutually satisfactory discount had been agreed on.[16]

[14] Kansas City is used rather than Chicago because changes in the major wheat-producing areas and in the normal lines of movement of the commodity have left Chicago with a vestigial spot wheat market that no longer affords a good source of spot price quotations.

[15] The case of a merchant or processor deserves to be considered rather than that of someone not in such a business, who might buy merely for storage, because merchants and processors gain auxiliary benefits from having stocks on hand that give them a competitive advantage in storing. Their competition for the returns available from storage leaves little opportunity for profitable storing as an independent enterprise.

[16] This is the normal procedure in connection with spot sales of wheat at Kansas City and at other markets with active futures trading. The actual bargaining on July 2, however, would have been in terms of premium or discount in relation to the price of

The fact that on September 4, No. 2 Hard Winter wheat sold at a discount under the September future, though it is the grade of wheat currently deliverable on the future, is accounted for by the fact that the spot price applies to wheat "on track," requiring additional expenditure to get it into a warehouse.[17] Wheat was then moving into commercial storage on a large scale because of heavy marketing by producers.

On September 4, our grain merchant or processor would probably not have sold the wheat he bought earlier, but instead would have bought more wheat. If he did that, and held until December 1, the results, in cents per bushel, would have appeared as follows:

Quotation	Date and Price Sept. 4	Dec. 1	Gain or Loss
Spot No. 2 Hard (low)	232½	252	......
December future	238¼	252	......
Spot premium	−5¾	0	+5¾ (gain)

In this case the spot price of the cheapest deliverable wheat came, on December 1, to exact equality with the price of the December future, and the gross return for storing the wheat was exactly what might have been expected, on September 4, from the fact that such wheat was then selling at a discount of 5¾ cents under the price of the December future.

the *July* future, the prospective hedger bearing in mind the prevailing discount of the July future under the September.

Since the gain or loss from hedging calculated in such tabulations as that in the text above depends only on the spot premiums, the prices included in the tabulations are no more than interesting collateral information. The spot premium or discount for a specified quality of the commodity rarely changes much during the course of a day or even a week. With regard to the futures prices, however, it is pertinent to note the time of day. Those used here are the closing prices for the day. The spot prices are closing prices of the future currently being used as a basis for spot sales, plus the quoted premium for lowest quality No. 2 Hard Winter wheat. The source is the Kansas City *Grain Market Review*, which quotes also daily high and low spot prices for the various grades, in which the low quotation for each grade is obtained by adding the premium for lowest quality wheat in that grade to the lowest price of the future for that day.

[17] Sometimes the spot price on track in a delivery month falls to a considerable discount under the near future because of lack of warehouse space for economical storage. The spot price on track tends to be at a *discount* under the price of a current-month future, which is then also effectively a spot price, when the prevailing direction of movement of the commodity is into storage; it tends to stand at a premium over the future when the prevailing direction of movement is out of storage. Moreover, the spot quotations for the cheapest wheat of deliverable grade may represent wheat of slightly better quality than that which will be delivered on futures contracts. To be graded No. 2, wheat must meet all of several requirements; the wheat delivered on futures contracts may be at or near the minimum in all respects when the cheapest wheat on which spot quotations are available is close to the minimum in only one of the grade requirements.

In these calculations we have left out of account the possibility that a merchant who bought at a discount of 5¾ cents on September 4 might have got wheat of a little better than minimum No. 2 quality—wheat which might have been sold on September 4 at a discount of, say, 5½ cents, rather than 5¾ cents, if the seller had been willing to look farther for a buyer. And we have ignored the possibility that on December 1 the merchant might have sold at a premium of ½ cent over the December future by virtue of the slightly superior quality of the wheat, and by finding a buyer who did not choose to shop around enough to get the best bargain possible. In other words, we have left out of account sources of normal *merchandising* profits.

On December 1 a merchant or processor may seem to have had no incentive for longer holding of wheat for which he had no immediate need. The spot price then was on a par with the December future, and at a premium of 1 cent over the price of the May future. But let us suppose that he continued to hold, with a hedge in the May future, and see what would have happened if he held until May 1. Though we imagine that the wheat is already in storage, we may make the next calculation as though it concerned a new purchase:

Quotation	Date and Price Dec. 1	May 1	Gain or Loss
Spot No. 2 Hard (low)	252	247¼	......
May future	261	238¼	......
Spot premium	+1	+9	+8 (gain)

This time a merchant would have gained a gross return of 8 cents per bushel from storage. It would have been in part a windfall profit, since he had no advance *assurance* of obtaining it; but he would have gained it on a quite conservative venture. He was well assured of not losing more than 1 cent per bushel (because the spot wheat that he held would surely sell at as high a price as the May future at some time in May), and he could count with virtual certainty on spot wheat going to a substantial premium over the price of the May future at some time between December and May.[18]

As of May 1, there remained no prospect of profit from continued storage of wheat during that crop-year, unless perhaps for a few days more. Before the end of the month, the spot premium, based on the

[18] One of the indications of this prospect was the fact that spot wheat had already reached a premium of 1 cent over the May future by December 1. The cause, of which any holder of large wheat stocks would have been well aware, was the holding by growers of some 300 million bushels or more under nonrecourse loans offered by the Commodity Credit Corporation.

May future, would have to fall from 9 cents to near zero.[19] Moreover, the spot price on May 1 was at a premium of 18 cents over the July future, and that premium should be expected to fall to zero or below by July 1. The outcome, if a merchant in fact held any wheat in storage from May 1 to July 1, was as follows:

Quotation	Date and Price May 1	July 1	Gain or Loss
Spot No. 2 Hard (low)	247¼	218½	......
July future	229¼	225	......
Spot premium	+18	−6½	−24½ (loss)

Probably some merchants did store a little wheat from May 1 to July 1, hedged in the July future, and did take the loss per bushel indicated by the above calculation. Grain merchants, like operators of retail stores, must try to keep adequate stocks on their shelves to serve their customers. But a merchant who hedged would have seen clearly on May 1 that any wheat that he might continue to hold until July would involve a loss, as surely, though not so completely, as would Christmas trees held until December 26.

The foregoing examples of hedging tend in one respect to be a little misleading; spot premiums do not always follow so obviously logical a pattern through the course of a crop year as they did in 1951-52. If spot wheat in July, were regularly, in all years, at a moderate discount under the September future, and if spot wheat, in September, were always at a large discount under the December future, and spot wheat in May always at a large premium over the July future, merchants and processors would have less need than they do for futures markets.[20] They would then have no need to watch spot-future price relations in order to judge when to accumulate stocks, and when to draw them low. But our purpose at the moment is merely to see how hedgers use spot-

[19] Not necessarily to zero, because deliveries on futures contracts would consist of wheat in public elevators; in May, wheat on track tends to be worth more than the same quality of wheat in a public elevator because it is already loaded in a freight car and ready to be moved to wherever it is wanted.

[20] When spot wheat in May is at a premium over the July future, it is not because the new wheat crop—coming to market in large volume by July—is expected to be large, but because current supplies of old wheat are scarce. (In May 1952 the scarcity applied only to commercially available supplies, being a result of the large holdings of wheat by the Commodity Credit Corporation in connection with its price-support operations.) On the subject of "inverted" intertemporal price relations in general, see Holbrook Working, "Theory of the Inverse Carrying Charge in Futures Markets," *Jour. Farm Econ.* (Feb. 1948), XXX, 1-28; "Professor Vaile and the Theory of Inverse Carrying Charges," *Jour. Farm Econ.* (Feb. 1949), XXXI, 168-72; and "The Theory of Price of Storage," *Am. Econ. Rev.* (Dec. 1949), XXXIX, 1254-62.

futures price relations as a guide in inventory control, thereby earning a return for holding stocks that must be stored by someone. We may reasonably avoid being led here into discussion of the frequent effects on spot premiums produced by exceptional export demand, by governmental price supports, or by unusual holding disposition on the part of producers.

We should now note three facts concerning hedging. First, contrary to a common impression, hedging of the sort here considered is not properly comparable with insurance. It is a sort of arbitrage. We shall consider later an example of conditions under which hedging may in fact be profitably compared with insurance, but such conditions obtain for only a small proportion of the hedging that is done on futures markets. Most hedging is done in the expectation of a change in spot-future price relations, the change that is reasonably to be expected being often indicated quite clearly by the current spot-future price relation.

Secondly, hedging does not eliminate risks arising from price variability. Risk is less than on stocks held unhedged, but it still exists. When the commodity involved is of quite different quality than that represented by the future, or in a location remote from that to which the futures price relates, the risks assumed by hedgers tend to be much larger than is suggested by the examples given here.

And thirdly, hedging is not necessarily done for the sake of reducing risks. The rôle of risk-avoidance in most commercial hedging has been greatly overemphasized in economic discussions. Most hedging is done largely, and may be done wholly, because the information on which the merchant or processor acts leads logically to hedging. He buys the spot commodity because the spot price is low *relative to* the futures price and he has reason to expect the spot premium to advance; therefore he buys spot *and* sells the future. Or in the case of a flour miller, he sells flour for forward delivery because he can get a price that is favorable *in relation to* the price of the appropriate wheat future; therefore he sells flour *and* buys wheat futures. (Here the arbitrage, it may be noted, is between two forward prices, that for flour and that for wheat.)[21]

Incidentallly, recognition of the fact that hedging may be done purely as a logical consequence of the reasoning on which the hedger acts (reasoning, for example, that the spot price is low relative to the

[21] Two instructive explanations of hedging written by hedgers themselves, such as are not often found, are: Ellis D. English, "The Use of the Commodity Exchange by Millers," *Proceedings, Fifth Annual Symposium* (Chicago Board of Trade, 1952, mimeo.) pp. 22-29; Virgil A. Wiese, "Use of Commodity Exchanges by Local Grain Marketing Organizations," *ibid*, pp. 108-16.

future) rather than from any special desire to minimize risks, helps to explain why many dealers and processors sometimes hedge and sometimes do not. As we have remarked, merchants and processors, even though they hedge, have need to keep informed on conditions that affect the price of the commodity and they may often have opinions on prospective price changes. If a merchant is accumulating stocks at a time when spot premiums are low—his most reliable basis for such action—and if at the same time he is fairly confident of an advance in futures prices as well as in spot premiums, why should he not carry the stocks unhedged, if he can afford to take some extra risk?

Perhaps the main reason that hedging, as commonly practiced on futures markets, has been so widely misunderstood and misrepresented is that economists have tried to deal with it in terms of a concept that seemed to cover all sorts of hedging. This would be desirable if it were feasible, but the general concept of hedging as taking offsetting risks wholly, or even primarily, for the sake of reducing net risk, serves so badly as applied to most hedging on futures markets that we need another concept for that most common sort of hedging. To put it briefly, we may say that hedging in commodity futures involves the ***purchase or sale of futures in conjunction with another commitment, usually in the expectation of a favorable change in the relation between spot and futures prices.***

An unfortunate consequence of the prevalent misconception of hedging has been that, while it has correctly credited futures markets with allowing merchants and processors to curtail their risks, it has diverted attention from a service of probably larger economic importance. Merely by supplying simultaneous quotations applying to various subsequent dates, futures trading tends to promote economically desirable control of stocks; and futures markets, through their use for hedging, make the holder of stocks sharply aware of any losses that must be expected from carrying unnecessary stocks in times of relative shortage of supplies, and provide assured[22] returns for storage over periods when there is a surplus to be carried. A merchant or processor with warehouse facilities will undertake storage in response to prospect of a 10-cent per bushel gain from carrying hedged stocks about as readily as he will undertake storage in response to an offer of 10 cents per bushel as a fee for storing government-owned grain. Indeed he may undertake storage for the return promised by hedging more willingly than for the fee, because the stocks that he holds hedged need be carried only as long as he wishes, and can be a source of convenience or of profit in connection with his merchandising or processing business. The argu-

[22] Though subject to some risk, as we have seen.

ment often made that management of reserve stocks of commodities should be a governmental function rests in large part on ignorance of the effectiveness with which the hedging facilities of futures markets assure private carrying of stocks in about as large a volume as can be justified on purely economic grounds.[23]

The claim sometimes made by able economists[24] that prices of such storable commodities as wheat, corn, and cotton fluctuate excessively because stocks are accumulated at wrong times, and not accumulated when they should be, seems also a consequence, indirectly, of the prevalent misconception of hedging. Mismanagement of stocks by nonhedgers would have to be on a very large scale to produce an over-all tendency toward perverse stockholding in any commodity with a futures market much used for hedging.[25]

IV. *Price Fluctuations*

Futures trading tends to emerge and persist especially in commodities which are subject to exceptionally large price fluctuations, arising from unpredictable variations in production, from other supply uncertainties, and from relative inelasticity of consumption demand.[26] Susceptibility of a price to large and unpredictable changes tends to stimulate hedging, and therefore futures trading, whether handlers of the commodity seek insurance against the risks of price change, or are led into hedging merely because they find spot premiums a more reliable guide to inventory control than are the prices themselves. (The relative superiority of spot premiums as such a guide depends of course on the price variability.) On this account, the fact that prices of commodities which have

[23] If considerations of national defense warrant the carrying of commercially uneconomic stocks of a commodity, government should of course assume the responsibility and the financial burden of carrying such excess stocks.

[24] For example, T. W. Schultz in *Production and Welfare of Agriculture* (New York, 1949), pp. 172-74.

[25] The hypothesis of perversity of stockholding tendencies is not supported by any statistics that I know, but is contradicted by them. Of particular interest is the fact that in the years when one could speak realistically of a world wheat market, the countries in which year-end (June 30) stocks of wheat varied in rational correspondence with world wheat supplies were the countries where hedging was practiced on a substantial scale. In most countries, year-end stocks of wheat varied little, and primarily with size of the previous *domestic* crop. Britain, with a futures market but with only small storage facilities, contributed little to the carrying of world wheat surpluses. Canada contributed more; and the country which most consistently carried large stocks at the end of any year of world wheat surplus, and reduced stocks to a minimum in times of world wheat shortage, was the United States. *Cf.* Holbrook Working, "The Changing World Wheat Situation," *Wheat Studies* (Sept. 1930), VII, 433-52.

[26] No reference is made here to changes in demand as a cause of *exceptional* price variability, because those demand changes which contribute to price instability of staple commodities are mainly of the sort that affect all sensitive prices similarly.

futures trading are found to be more variable than most other prices gives no ground for supposing that futures trading is a *cause* of the exceptional price variability.[27] It is none the less pertinent to raise the question whether existence of futures trading has a stabilizing or destabilizing influence on prices, and to seek some objective evidence on the question.

The results of attempts to determine whether prices of commodities that have futures trading fluctuate more or less than they would in the absence of futures trading have been generally inconclusive. Even if clear proof were given that futures trading tended somewhat to restrict price fluctuations, it might still be true that futures prices fluctuate too much. Some criterion is needed for an absolute test by which to determine whether the price fluctuations that occur are excessive, or are, in the main, rational and desirable responses to changing economic conditions and information.

A few years ago I suggested that such a test might be developed from the consideration that prices of durable goods (and especially futures prices) reflect expectations.[28] These expectations are always subject to error, but the errors of expectation might, in an *ideal* market, be only such as must arise from uncertainties inherent in the economic situation. That is, the price of May wheat, for example, might fluctuate, and yet be always the best estimate that could be made at the moment of what the price of wheat should be next May. *Excessive* price fluctuations might be measurable as the amount of fluctuation that occurred over and above the amount attributable to unpredictable changes in the economic situation. Unpredictability of change would thus be taken as the ideal in price behavior.

This idea has been pursued, and appropriate statistical methods have been devised for making the suggested tests.[29] For technical reasons the new approach to the problem of testing price behavior has been applied first to appraisal of the frequent and sometimes large price fluctuations that occur on active futures markets during the course of a day. The results indicate some departure of actual price behavior from the ideal, as was to have been expected, but only slight departures; the observed

[27] It appears sometimes to be so taken, nevertheless, even by economists who would be expected to see the fallacy of such an inference.

[28] "The Investigation of Economic Expectations," *Papers and Proceedings, Am. Econ. Rev.* (May 1949), XXXIX, 158 ff.

[29] The tests involve in principle the measurement of serial correlation among price *changes*, which in practice must be present to some extent if price changes are predictable (that this is so may seem obvious, but the proof requires more space than is available here). The statistical measures used have been especially devised to be more sensitive than serial correlation coefficients to the sorts of departure from randomness of change that are to be expected in price series. They have also an advantage of economy in use.

price fluctuations were for the most part such as should occur purely from unpredictable changes in price prospects.[30]

In an ideal futures market in which the price of May wheat, for example, was at all times the best possible estimate of what the price would be next May, no speculator would be able to consistently make money, and the speculation necessary to maintain even an approximation to ideal price behavior would tend to vanish. Speculative profits that are not purely the result of chance must rest on ability to anticipate price changes with some degree of reliability, whereas if a futures price were always the best possible estimate of price at a later date, its changes would be entirely unpredictable.[31] Since many professional traders do make money in actual markets with some degree of consistency, it is evident that they are able to anticipate price changes with some approach to reliability and hence that the price behavior is not ideal. Study of the nature of the price fluctuations that professional traders are able to anticipate may therefore give the best clue to the nature of the imperfections of actual speculative markets—the predictable price fluctuations that would be absent in an ideal futures market, but that are present in actual markets.

Perhaps the largest class of professional traders is that of "day traders"—those who operate primarily on intraday price fluctuations, and who end each day neither net long nor net short in appreciable amount.[32] One such trader in cotton had the following record over a two-month period chosen substantially at random.[33]

Month	Number of Transactions[a]		Average Daily Sales (million pounds)	Gross Profit[b]		Number of days with	
	Total	Per day		Cents per lb.	Per-cent	Gain	Loss
February	1701	77.4	4.9	.0167	.042	15	7
March	1343	64.0	4.2	(c)	(c)	13	8
Total	3044	70.8	4.6	.0093	.023	28	15

[a] Purchases and sales.
[b] Per pound or per dollar of sales.
[c] Infinitesimal; a gross profit of $187.00 on $35 million of sales.

[30] The relative amount of departure from ideal behavior has been measured numerically, but it is not possible to summarize the results meaningfully in brief.

[31] Price changes would always be consequences of new information, unavailable as a basis for prediction before the price change occurred.

[32] Professional traders specialize to such an extent as to make classification meaningful, and the intraday traders especially tend to concentrate on one type of trading.

[33] The record was obtained for this individual because he was regarded as a representative, successful, day trader. The period was simply the two months ending at the time (in 1952) when he was interviewed.

During the two months of the record, this man averaged nearly 70 trades per day, which is at the rate of about one every four minutes. His purchases were not bunched during the parts of the day when prices were low, nor his sales bunched during the parts of the day when prices were high, but purchases and sales were distributed throughout the price range of the day.[34] He made money simply by managing to have his purchases, on the average, at prices a little lower than the prices of his sales. On the days on which he made money, his gross profits averaged $\frac{1}{30}$ cent per pound. Since the cotton price was about 39 cents per pound, his gross profit on the days when he made money was about 82 cents per thousand dollars worth of cotton that he bought and sold. Net profits, after paying commissions and other business expenses, were substantially less.

The gross profits calculated above are on trading during only the 28 days out of 43 on which he made money. On 15 out of 43 trading days during the two months, he lost money. Such a result might not be surprising in the case of a man who made only a few trades each day, but 70 trades per day—about 35 purchases and 35 sales—gave much opportunity for successes and errors to offset within a day. Nevertheless, he lost money on more than one day out of three, on the average. This is not the sort of experience that most people imagine successful professional traders as having.

Because profits were so uncertain and variable, a calculation of the average rate of profit of this trader over even a two-month period may not give a very reliable indication of the normal profit expectation for such a trader. The figures, for whatever they may be worth, show that in the first of the two months, his gross profit averaged $\frac{1}{60}$ cent per pound; in the second of the two months, losses nearly equalled gains—his total gross profit for the month would not have covered the commission charges he often paid on a single day's trading. For the two months together, his gross profits averaged about 23 cents per thousand dollars worth of cotton sold—less than one-fortieth of one per cent. Doing such business, he could make a living only by dealing in great quantities of the commodity; he bought and sold an average of over nine thousand bales of cotton—4.6 million pounds—per day.[35]

Another day trader, who was not primarily a true scalper, but sought principally to trade on larger price fluctuations within a day than those

[34] This was ascertained by separately tabulating purchases and sales each day in frequency distributions according to price.

[35] Incidentally, the fact that a gross profit of 23 cents per thousand dollars worth of purchases and sales could permit any net profit at all is striking evidence of the main technical characteristic of futures trading, the economy with which transactions can be made.

on which pure scalpers operate, found that his gross profits during the previous seven months, on the Chicago Board of Trade, averaged 70 cents per thousand dollars of sales—seven hundredths of one per cent. The period beginning with January 1952 was one which he described as especially successful. Giving particular attention, as he did, to somewhat larger price fluctuations than occupy the pure scalper, he made relatively few trades per day in any one commodity, but a considerable total number because on most days he did trading in five or six different commodities.[36]

One might seek to get a large number of records such as those summarized above, and for longer periods, in order to arrive at a conclusion regarding *typical* behavior and profits of day traders, but it is clear from this and other evidence, that the typical day trader and his profits would prove about as elusive as the typical insurance salesman and his income. These records, by themselves, serve as warning that some popular concepts of the manner in which professional traders operate, and of the sources of their profits, may be quite mistaken. With other and more detailed records, they give valuable aid in interpreting results of statistical measurements of price behavior, made on the principle outlined above.[37] All of the evidence converges toward the conclusions that: (1) the price movements that day traders are able to anticipate with even moderate reliability are usually small relative to the total price range for the day; (2) the reliability of their judgment is rather low; and (3) the over-all effect of their trading operates strongly toward "smoothing" the course of prices, helping to make intraday price fluctuations conform closely to our criterion of ideal behavior.

These conclusions bear directly on the question whether the price fluctuations that occur in futures markets within the day tend to be excessive or not. The question is not inherently a very important one because, though price fluctuations in the two months of the illustration covering cotton resulted in an average difference of 0.4 cent per pound between the lowest price and the highest price each day, it would not matter greatly if fluctuations of such magnitude did occur without good reason. The conclusions reached are more important than the specific question to which they apply, because they indicate reasonableness in just that trading and those price fluctuations which may be thought most likely to be unwarranted.

On the question whether the larger price fluctuations that occur over longer periods are in the main warranted, the best evidence that I

[36] A feat comparable to that of playing several games of chess simultaneously, blindfolded.

[37] A full report on the research will be published shortly.

can yet cite[38] is essentially subjective and inconclusive. It is the evidence which led me to question the common assumption, and to try to measure the amount of "excessive" fluctuation that is present in "speculative" prices. During the twenty years of publication of *Wheat Studies* by the Food Research Institute, members of the research staff periodically studied the recent fluctuations of wheat prices and sought to interpret them as warranted by current developments, or unwarranted. For much of that period we sought three times a year to appraise price prospects for the next several months. Everyone concerned with these efforts gained a great respect for the rationality of the price behavior observed. During twenty years some price movements occurred which it seemed possible to appraise at the time as ill-founded or excessive, but these were exceptional. Only rarely did it seem possible to anticipate subsequent price movements with confidence on the ground that the current price appeared unjustifiably high or low.

As regards price changes from year to year, it is entirely clear, as noted in the previous section, that futures markets contribute substantially toward desirable adjustment of stocks carried from one year to another. Whether or not they produce as much flexibility of storage as is desirable they at least operate in that direction.

V. *Costs of Hedging*

We turn now from topics on which there has existed major disagreement, springing largely from imperfect or mistaken concepts, to examine a new, or at least largely neglected, idea that illuminates hitherto obscure aspects of futures trading. The idea came to me through puzzling over two questions:[39] Why does futures trading in a commodity tend to concentrate largely or wholly in one exchange? And why is futures trading on any one exchange usually confined to a single set of contract specifications for a commodity, rather than distributed among several contracts, representing different qualities of the commodity? Hedgers, it may seem, would be best served by having numerous futures exchanges, and several different futures contracts in each, so that hedges could be placed always in a future that applied to a quality of the commodity corresponding closely with that being hedged, and to a location at or near that of the hedged stocks. The answer seems to lie in a cost of hedging.[40]

[38] Pending completion of research under way.

[39] And also from what we learned in the study of intraday price behavior referred to above.

[40] I thought at first that the answer lay in a somewhat capricious behavior of speculators, coupled with a necessity for hedgers to use those exchanges and contracts in which there was sufficient speculation to carry the hedges. But, when this argument was advanced in a paper dealing with a special case ("Western Needs for Futures Markets," mimeo., Food Research Institute, 1952) it drew objections that seem fatal.

There are at least two significant elements to be considered as parts of the cost of hedging.[41] The most obvious one is the commission charge that must be paid on futures transactions.[42] Futures trading must operate with great efficiency to keep this charge so low as not to discourage hedging seriously, since much of the advantage that a hedger gains, as we have seen, comes from a guidance in inventory control that is available without actually placing hedges. In addition to commissions, there is a cost of hedging that can be much larger, arising from what may be called the bivalence[43] of market price. The contribution of price bivalence to costs of hedging declines sharply, as we shall see, with increase in the volume of business done on an exchange, and it declines also with increase in the volume of business in a particular futures contract. It has, therefore, just the characteristics needed to explain observed tendencies in the concentration of futures trading.

Our awareness of the fact that there are usually two (or more) prices for a commodity unit of specific quality in any market at a given time is dulled by the convention of treating wholesale and retail prices as though they were registered in separate markets. But of course they are in fact two separate prices within the same market, differing because of a service that (at least presumptively) goes with the commodity when it is sold at retail.

When, as in the case of houses, for example, the circumstances of trade do not favor distinction between wholesale and retail prices, there remain price differences according to the conditions of sale. Even under stable and well-known market conditions the man who chooses to sell without much search for someone who particularly wants a house like his, takes one price, and the man who chooses to buy without much "shopping" pays a higher price. The difference between the two prices need not represent any exploitation of bargaining weakness, but only a fair margin for an intermediary. Or in a country grain market, a merchant may buy wheat of a farmer at one price and sell it almost immediately to a poultry raiser at a higher price. The difference, again, is likely to represent only a return for service, small in this case because a poultry raiser who had reason to believe that the difference would be large would seek out the farmer and buy from him directly.

In terminal commodity markets (and in security markets) there ap-

[41] One might choose to regard potential speculative profits foregone as a cost of hedging; but though a potential hedger may wisely take account of potential profits from speculation in deciding whether to hedge or not, I think that treating foregone speculative profits as a cost of hedging would not be the best way of taking them into account.

[42] And if the hedger maintains an exchange membership for the sake of the consequent saving in commissions, there are membership expenses to be counted as part of the cost.

[43] Using "bivalent" to mean "two-valued" rather than, as in chemistry, "having a value of two."

pear the same sort of price differences, though smaller, that provide the margin on which merchants in such markets principally operate. In large central markets the small price differences which provide these margins cannot be realistically treated as differences between a wholesale and a retail price. They tend then to be clearly reflected in price quotations only through differences between "bid" and "asked" prices.

A merchant who hedges usually finds his situation in the futures market the opposite of that which he enjoys in the spot market. In his spot dealings he buys at bid prices and sells at the higher asked prices (a fact that we ruled out of consideration in our illustrations of hedging, because we were there concerned with storage and its returns). In his futures dealings, on the contrary, he tends to buy at asked prices and to sell at the lower bid prices. At least he prefers to do so, unless the margin between bid and asked prices is so wide that he is forced to "shop" in the futures market, as well as in the spot market. The merchant is paid for his services to processors and exporters by *receiving* an asked price, and he in turn is willing to pay for corresponding services of dealers in futures by buying often of them at asked prices.

The dealers in futures from whom a hedger often[44] buys at asked prices, or to whom he sells at the lower bid prices, are the so-called scalpers, or other day traders.[45] We saw in the last section some evidence on the very narrow margin which such dealers in futures take. From that evidence it would appear that their margin is of the order of $\frac{1}{14}$ to $\frac{1}{40}$ of one per cent. Those figures, however, represent averages that are somewhat "watered down" by the effects of transactions between day traders themselves.[46]

Another way of estimating the dealer's margin that a hedger tends to pay on his futures transactions is from the profit margin that scalpers try for. In wheat on the Chicago Board of Trade, this has been typically $\frac{1}{8}$ cent per bushel, or a little over $\frac{1}{20}$ of one per cent at recent prices. An estimate so derived, however, tends slightly to overstate the margin

[44] "Often," because the purchase or sale of futures made by a hedger does not necessarily go through the hands of an intermediary scalper.

[45] A scalper, in the strict sense, may be characterized as always willing to either buy at $\frac{1}{8}$ cent below the last price or to sell at $\frac{1}{8}$ cent above the last price (or at such other difference from the last price as market conditions permit); he operates purely on the difference between bid and asked prices, as ordinarily understood. Other professional day traders perform an essentially similar function, but at least partially with respect to somewhat larger price differences. Bid and asked prices tend to be farther apart for large quantities than for small quantities of a commodity.

[46] The first of any pair of transactions by a scalper is made in the belief that he is buying "below the market" from an urgent seller, or selling "above the market" to an urgent buyer. But scalpers often find that they have misjudged the market, or that it has turned against them immediately after such an initial transaction. Then they seek to make the offsetting transaction quickly, often at a loss, and often to another day trader.

actually paid by a hedger, because hedgers benefit from scalpers' mistakes of judgment.

A third basis for estimating the margin that hedgers pay on their futures transactions is afforded by data showing that a loss of 0.21 cents per bushel was taken by processors and terminal grain merchants on 109 million bushels of wheat futures bought (and sold) over a nine-year period.[47] If the futures transactions were virtually all hedging transactions, as may reasonably be assumed, this indicates a hedging cost, in addition to commissions, of slightly over ⅕ cent per bushel, or less than ⅕ of one per cent of the average wheat price over the period. This is a cost figure, however, that includes speculative profits foregone[48] (or, to put the same thing in another light, that includes any return to speculators for carrying the hedges). And it is in any case a cost per bushel bought and sold, whereas the foregoing estimates of scalpers' margins might tend to be paid on purchases and on sales alike. So there is at least no evident inconsistency between these data and the previous estimates of scalpers' margins.

Whether scalpers' margins are less than ⅒ per cent, or as much as ⅕ per cent, a merchant or processor can afford that cost in addition to

[47] The data, from Blair Stewart, *An Analysis of Speculative Trading in Grain Futures*, U. S. Dept. Agric., Tech. Bull. No. 1001 (Oct. 1949), Table 27 (the calculations of loss per bushel are mine) are as follows:

Business	No. of Firms	Transactions (thousand bushels)	Loss (thousand dollars)	Loss per Bushel (cents)
Terminal grain merchants	45	76,054	174	0.23
Processor	44	33,407	55	0.17
Total	89	109,461	229	0.21

Similar data, from the same source, on hedging transactions amounting to 9.6 million bushels by 33 country and subterminal grain merchants, show a loss of 2.6 cents per bushel on their transactions in futures. The magnitude of this loss indicates that more of the hedges against wheat stocks by this group of dealers were carried in periods of rising prices than in periods of declining prices. The classes of grain merchants involved commonly practice discretionary hedging, and those represented in the data apparently chose to hedge at such times that they "protected" themselves against profits from price increase somewhat more than against losses from price decreases. The results tabulated above for terminal grain merchants and processors may also be affected to some extent by the practice of discretionary hedging, though it is much less prevalent among such wheat handlers than among country and subterminal merchants.

[48] Which we wish to leave out of account here, as previously noted. It is scarcely possible to deal with them in general terms, except in such a global average as the one just given, because their magnitude depends so much on special circumstances, including the knowledge and judgment of the hedger.

commission charges of, say, $\frac{1}{10}$ of one per cent.[49] His own dealer's or processor's margin, though small, is usually several times the total of these costs of hedging.

In small and relatively inactive futures markets, however, scalpers must take much wider margins than in the circumstances to which the foregoing data apply.[50] They must do so primarily because their volume of business is restricted to perhaps half-a-dozen transactions per day, or less, as compared with the 70 transactions per day of the cotton trader cited. And, secondly, they must do so because their risks are greater. In an active futures market, a scalper can usually buy and resell within the space of a few minutes, running little risk that some change in news will involve him in a serious loss. In inactive markets, purchase and sale may be separated by hours, or the scalper may buy today and have to wait until tomorrow to sell, running correspondingly large risks of loss from unpredictable developments. And because scalpers must take wider margins on inactive markets, hedging on them is more costly than on active futures markets.[51] Consequently, if a hedger has two futures markets to choose between he tends to do his hedging in the more active one.[52]

The element of hedging cost that arises from scalpers' margins, or from the inconvenience and price disadvantages incurred in the absence of scalpers, explains the restriction of trading almost universally to a single futures contract on one exchange, in the same way that it explains the tendency for futures trading in a commodity to concentrate mainly or wholly on some one exchange. When two futures contracts are offered for trading applying to different descriptions of the commodity, trading tends to become more active in one of them than in the other; scalpers' margins then rise on trading in the less active contract, leading to

[49] Commission charges are not fixed as a percentage of the price; expressed so, they depend on the price, and they vary rather widely among commodities and exchanges, and according to whether the transaction is for a member of the exchange or a nonmember. The figure of $\frac{1}{10}$ of one per cent is representative of commissions for commodities and exchanges with a fairly large volume of business and low commission rates, and is therefore applicable to most of the hedging that is done.

[50] Part of the difference in gross profit obtained per $100 of transactions by the two day-traders for whom data were given, is accounted for by the fact that the second trader operated mainly in commodities which have only moderately active trading.

[51] At some point not very far down on a scale of diminishing market activity, it becomes impossible to conduct scalping as a specialized form of trading and to make a living at it. Then the scalping function is performed by some speculators, and the distinction between scalping and speculation becomes blurred.

[52] The preference, as commonly expressed in trade circles, is for a "broad" market as against a "thin" market. This terminology expresses the fact that size as well as frequency of transactions is important. On the supposition that most readers would understand "active" to mean about what a trader calls "broad," I have thought it unnecessary to introduce the trade terminology into the exposition.

further concentration of hedging, and of other trading, in the more active contract. For example, after five years of use of the Californian wheat contract for futures trading at Liverpool, trading was initiated also (in 1891) in an American Red wheat contract. The latter proved the more popular, and in spite of the important difference between the two kinds of wheat, quickly drew trade away from the Californian contract to such an extent that trading in the Californian was presently abandoned.[53] Maintenance of futures trading in two descriptions of a commodity on any one exchange was found generally impractical early in the history of futures trading, and has rarely been tried in recent years.

These conclusions have some interesting implications. If we are correct in inferring, as seems necessary, that hedgers' responses to cost differentials account for the observed tendency toward concentration of futures trading dominantly or wholly in some one exchange, and wholly in some one contract on each exchange, it follows that hedgers are as a rule unwilling to pay for superior hedging facilities. Does this mean that hedging is usually considered worth while only if it is very cheap—that its advantages are really not very great? Or does it mean something else?

VI. *"Insurance" Hedging*

The question posed above may well be considered in a concrete situation. Grain merchants and flour millers in the Pacific Northwest of the United States have long sought to gain and hold the advantages of futures trading in a contract well suited to their needs. Largely because of the great distance between that area and the main wheat-producing regions of the country, wheat prices in the Pacific Northwest are only loosely tied to prices at Chicago or Kansas City. When the Chicago futures price represents hard wheat, as has commonly been the case,[54] the important quality difference between hard wheat and the soft wheat typical of the Pacific Northwest contributes to disparity of movement between Chicago and Pacific Northwest wheat prices. No. 1 Soft White wheat in Portland or Seattle may sell at 20 to 30 cents per bushel under the spot price of contract wheat at Chicago, or it may sell at 10 cents per bushel, or more, above the Chicago price. Chicago futures consequently afford a very imperfect hedge for soft wheat in the Pacific Northwest.

Efforts to provide good hedging facilities for Pacific Northwest wheat have included maintenance, over many years, of futures trading at

[53] *Cf.* Holbrook Working and Sidney Hoos, *op. cit.*, p. 144.

[54] Though during the last few years the Chicago future has most of the time been effectively a soft-winter wheat future because the deliverable soft wheat was cheaper than deliverable hard wheat.

Seattle and, until 1942, at Portland also. These markets did not flourish,[55] and in 1950 the experiment was tried of providing for trading in North Pacific Coast wheat futures on the Chicago Board of Trade (with delivery on the Pacific Coast). The special Chicago contract failed to attract enough business to warrant its continued use; it seemed to serve only to draw business away from Seattle.

The volume of hedging in a Pacific Northwest wheat futures contract must necessarily be a fairly small fraction of total wheat hedging in the United States, because total wheat production in the area is only some 10 to 12 per cent of the national wheat crop.[56] Moreover, there is relatively less occasion for hedging in the Pacific Northwest than in most other areas of concentrated wheat production in the United States, because of abundant "country" storage facilities and a widespread disposition of growers to retain ownership of their wheat for considerable periods after delivery.[57] But even so, there could have been a lively business in Pacific Northwest wheat futures if commercial stocks of wheat in that area had not been held to such a large extent either unhedged, or hedged in futures markets of the Middle West, especially Chicago (and in the standard contract there).

Decision of a wheat merchant or processor in the Pacific Northwest to hedge in Chicago futures rather than in a Pacific Northwest futures contract is only a rather extreme example of the sort of decision often made by hedgers—to take inferior risk protection for the sake of a saving in cost. Even though he hedges in Chicago futures, a merchant or processor in the Pacific Northwest can still rely on prices of the Seattle futures for guidance in deciding whether to buy for storage or not to buy.[58] If he buys because the relation between the spot price and a Seattle futures price promises a return for storage, he may still decide to hedge in a Chicago future with only the result that he will take more risk, but not so much risk as though he held the wheat unhedged. In other words, the decision on where to place the hedge concerns only the insurance aspect of hedging.[59]

[55] See data on open contracts in wheat futures, by exchanges, in *Commodity Futures Statistics*, U. S. Dept. Agric., Stat. Bull. No. 107 (1952), pp. 5, 10, and earlier publications in the series.

[56] Opinions differ as to how the "Pacific Northwest" wheat area should be defined, but on any reasonable definition, it does not follow the boundaries of states, which are the units for wheat production statistics; consequently the production of the area can be stated only roughly.

[57] *Cf.* J. S. Davis, "Pacific Northwest Wheat Problems and the Export Subsidy," *Wheat Studies* (Aug. 1934), X, 377.

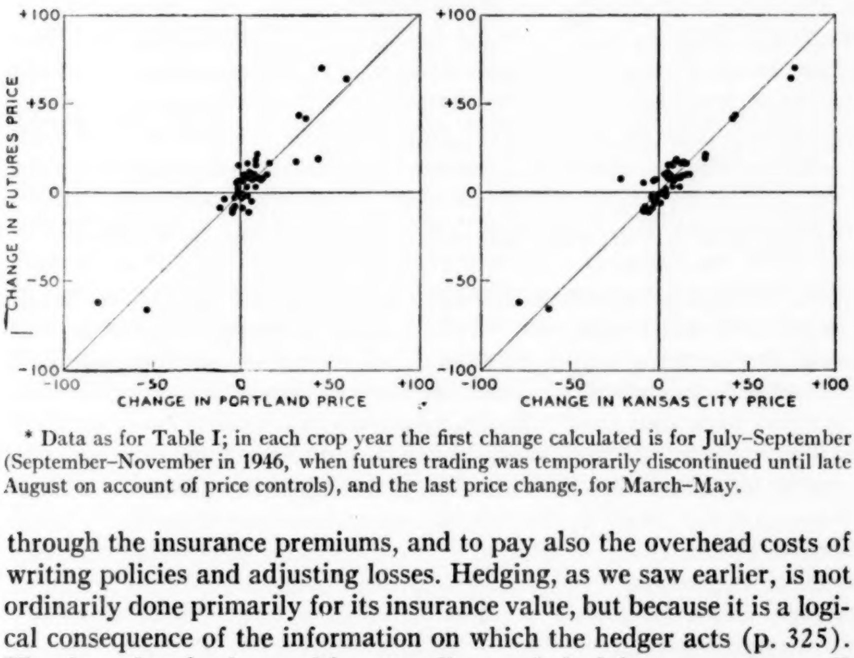
[58] Though of course, if too little hedging is left for the Seattle market, even the guidance afforded by its prices will be lost.

[59] Unless he expects a change in the relation between Seattle and Chicago prices, in which case the hedging problem becomes entangled with one of intermarket arbitrage.

A merchant or processor choosing between Seattle and Chicago futures as the medium for hedging that he has decided to do is in much the same position as though he were taking out casualty insurance, and choosing whether to take full coverage or coverage only for losses above some stated minimum. The choice he makes is whether to take coverage on all insurable risks, at a fairly high cost, or to insure only against serious loss, at a considerably lower cost. The chief function of insurance is to give protection against serious, crippling, loss. Carrying insurance against small losses that occur frequently is ordinarily poor business because the holders of the insurance have to pay the losses,

CHART 1.—RELATIONS OF TWO-MONTH CHANGES IN PRICES OF CHICAGO WHEAT FUTURES TO SIMULTANEOUS CHANGES IN PORTLAND AND KANSAS CITY SPOT PRICES, SEPTEMBER 1946 TO MAY 1952*

(cents per bushel)



* Data as for Table I; in each crop year the first change calculated is for July–September (September–November in 1946, when futures trading was temporarily discontinued until late August on account of price controls), and the last price change, for March–May.

through the insurance premiums, and to pay also the overhead costs of writing policies and adjusting losses. Hedging, as we saw earlier, is not ordinarily done primarily for its insurance value, but because it is a logical consequence of the information on which the hedger acts (p. 325). The fact that business risks attending such hedging are very small is none the less a valued consequence of such hedging; and when the costs of hedging in the futures contract logically indicated are excessive, businessmen naturally consider the possibilities of obtaining at least the insurance values of hedging at a moderate cost. The principal insurance value of hedging wheat stocks in the Pacific Northwest is supplied about as well by Chicago futures as by Seattle futures.

This interpretation of hedging deserves empirical support. Is a

TABLE I.—SPOT AND FUTURES PRICES OF WHEAT AND TWO-MONTH PRICE CHANGES, JULY 1947 TO MAY 1948*
(*cents per bushel*)

Date	Chicago Futures[a]		Spot Prices		Two-Month Price Changes		
	Dec.	May	Kansas City[b]	Portland[c]	Chicago Futures	Kansas City	Portland
1947							
July 16	237	...	234	219	....	....	....
Aug. 16	237	...	231	222	....	....	....
Sept. 15	279	...	275	256	+42	+41	+37
Oct. 16	302	...	306	281	+64	+75	+59
Nov. 15	299	286	301	300	+19	+26	+44
Dec. 16	309	296	304	293	+07	−02	+12
1948							
Jan. 16	...	305	311	291	+18	+10	−09
Feb. 14	...	235	235	212	−62	−79	−81
Mar. 16	...	239	249	237	−66	−62	−54
Apr. 16	...	252	249	243	+17	+14	+31
May 17	...	244	240	235	+05	−09	−02

* Prices for that trading day nearest the middle of the month on which quotations are available in all three markets, rounded to the nearest cent.
[a] Means of highest and lowest prices during the day, from Chicago *Journal of Commerce*.
[b] Weighted average of reported spot sales of No. 2 Hard and Dark Hard Winter wheat, compiled by U. S. Department of Agriculture from Kansas City *Grain Market Review*.
[c] Spot prices of No. 1 Soft White wheat, compiled by U. S. Department of Agriculture.

"poor" hedge reasonably comparable with insurance that covers losses above some stated minimum? Or is it more properly comparable with insurance covering only a *fraction* of the total loss; or with insurance carried with a company of uncertain financial status, that may turn out to be no insurance at all? The facts are well illustrated by Chart 1.

The meaning of the points plotted in the chart may be grasped readily by considering their relation to the illustrative data in Table I. The point, for example, that appears at the extreme lower left of each section of the chart, is plotted from the data for "two-month price changes" appearing in the table opposite "Feb. 14." These are price changes from December 16, 1947 to February 14, 1948. Over this two-month interval, the spot price at Portland fell 81 cents; that at Kansas City fell 79 cents; and the price of the Chicago May future fell 62 cents. Despite these severe price declines holders of wheat stocks of the indicated qualities at Portland and Kansas City, hedged in the Chicago future, would have lost only 19 and 17 cents per bushel, respectively.

Points on the diagonal line across the center of each section of the chart represents instances in which the spot and the futures prices

changed equally. Points to the left of this line represent instances in which the spot price fell more than the future price, or rose less. These were instances in which hedged stocks would have been carried at a loss, as in the specific case just considered. Conversely, points to the right of the diagonal represent instances in which a gain would have resulted from the carrying of hedged stocks. Nothing further need be said here about the way in which hedgers use futures quotations to judge in advance when the carrying of stocks is likely to prove profitable, but it is pertinent to note that Table I includes evidence that a hedger anywhere in the Middle West, at least, had reason to expect a loss if he carried stocks of wheat from December 16, 1947, to February 14, 1948. This prospect was indicated by the fact that on December 16 the price of the Chicago May future was 13 cents under the price of the December future.[60]

Amounts of gain or loss from carrying hedged stocks of wheat are indicated directly in Chart 1 by the horizontal (or vertical) distances of the plotted points from the diagonal line. The scatter of the points is greater, and therefore gains and losses were greater, for Portland wheat than for Kansas City wheat, though perhaps not so much greater as some would have expected. But the important fact for present purposes is that the points in the chart cluster as closely around (or are no more widely dispersed around) the diagonal lines near their ends than near their midpoints. There is no evident tendency for gains and losses on hedged stocks to have been larger when price changes were large than when price changes were small. Hedgers take some risks even though they can estimate fairly well the prospective gain or loss from storage, and a good deal more risk on wheat stored in Portland and hedged in Chicago than on wheat stored in Kansas City and hedged in Chicago. But in either case the amount of risk is substantially *independent* of the amount of price change. Hedging limits the amount of risk in substantially the same sense that insurance covering losses above a stated amount limits risk.

In the light of these facts, it is understandable that many hedgers should prefer a "poor" hedge that is cheap to a more nearly perfect hedge that is relatively expensive. The tendency for most futures trading in any commodity to converge in some one exchange, and to concentrate in some one contract there, is explained. There is even an implied suggestion that trading in barley futures, for example, may have died out in the United States because grain dealers chose commonly to

[60] This relation reflected a current high premium on spot wheat over the May future, indicative of prospective declines in spot prices relative to the May future throughout the United States east of the Rocky Mountains, but not necessarily on the Pacific Coast.

hedge their barley stocks in corn futures, at low cost, in preference to hedging in the small and relatively inactive barley futures market, where the costs of hedging were relatively high. And the fact that merchants and processors make such choices does not necessarily indicate that they put a low estimate on the value of hedging. Their valuation of hedging must be judged from cases where the only choice open is to hedge in a market where hedging is expensive, or not to hedge at all. In such cases the common choice is to hedge, as is evidenced by the vitality of small futures markets that have no larger competitor.

VII. *Summary*

To summarize, we began with a definition of futures trading that related it intimately to other commodity transactions, and emphasized economy as its major distinguishing feature. A language problem, we found, has promoted a false idea of contrast between futures trading and other commodity trading. Looking at the bases of futures trading, we saw them to lie more in utilization of the advantages of futures markets by merchants and processors, for hedging, than in the desires of others to speculate.

Hedging we found to be not primarily a sort of insurance, nor usually undertaken in the expectation that spot and futures prices would rise or fall equally. It is a form of arbitrage, undertaken most commonly in expectation of a favorable change in the relation between spot and futures prices. The fact that risks are less with hedging than without is often a secondary consideration. The prevalent tendency to regard curtailment of business risks as the main service of futures markets has diverted attention from their probably more important service of promoting economically desirable adjustment of commodity stocks, thereby reducing price fluctuations. The argument for governmental stockpiling rests heavily on a consequent false appraisal of the causes of price fluctuations.

In further consideration of the subject of price fluctuations, we stated an ideal of behavior of a futures price that permits objective statistical tests, and put in one sentence the gist of conclusions arising from application of such tests to the intraday behavior of futures prices. The statement of conclusions was prefaced by some data on the operations of two professional traders which reveal characteristics far from those usually imagined to exist. Pending further research based on the stated concept of ideal price behavior, inferences on the reasonableness of the larger fluctuations of futures prices, beyond the limits of one trading session, must still rest largely on subjective judgment, but there is considerable evidence that these larger price fluctuations may usually re-

flect substantially accurate appraisals of changing economic facts that should be accompanied by such price changes.

Finally, we inquired into the causes of some puzzling characteristics of futures trading and found them explained by hedgers' responses to cost differentials associated with the "bivalence" of market price. The responses themselves depend largely on the fact that even a "poor" hedge affords good protection of the sort for which insurance is mainly needed. It is like casualty insurance covering losses above some stated minimum.

ECONOMICS OF HIGHER EDUCATION

By Seymour E. Harris*

The Commission on Financing Higher Education (hereafter referred to as the Commission) recently released its Report, *Financing Higher Education in the United States* (hereafter referred to as *Report*) and ten research volumes, inclusive of a 500-page Staff Report (hereafter referred to as *Millett*), by John D. Millett, executive director. The Commission consisted of twelve members: college administrators (8), and businessmen and lawyers (4). The executive committee consisted of Messrs. P. H. Buck, F. A. Middlebush, and H. M. Wriston. No economist was included, though the problems were primarily economic, and no nonadministrative faculty member was included.

This Commission has written an important report, for which all should be grateful; and it has made available an abundance of materials which will greatly lighten the tasks of researchers in this field for many years. Both the Rockefeller Foundation and the Carnegie Foundation are to be congratulated for their generous donations for the work of this Commission, which made possible the provision of so much valuable material.

These eleven volumes offer an opportunity to discuss the vital economic issues confronting Institutions of Higher Learning (hereafter referred to as IHL). At the outset, however, we should note some peculiarities of the market for college students. First, many potential "buyers" are excluded from the market. Students have to meet minimum standards (*e.g.*, a high school diploma). Such restrictions limit the number of "buyers" of higher education. Second, buyers are not usually asked to pay the full costs of an education. The effect of "sales" below cost is to increase numbers seeking a college education, though the net effect here is not so great as is commonly supposed (except in medical schools, where a tuition of about $700 per year is to be set against a cost per year of about four times the tuition). For private institutions, the "subsidies" per year amount to about $120 and for public institutions about $260, or an average of almost $200. These sums are large in relation to *educational* costs (one-third of educational costs for private institutions, two-thirds for public), but much less relative to the $1,000 to $2,000 total cost in private IHL and a minimum of $1,000 for in-students at state IHL, plus in each instance the income foregone. Costs for nonresidents are less.[1] Education below cost is offered despite the fact that a college graduate can look forward to an income of $100,000 more than a noncollege graduate and a medical school graduate, $300,000 to $400,000 more.[2]

* The author is professor of economics at Harvard University.

[1] Figures calculated from *Millett*, pp. 140, 300-01, 389.

[2] *Cf.* E. Havemann and P. S. West, *They Went to College* (New York, 1952), p. 26, and my article "Why the Government Need not Subsidize Medical Schools," in *The Reporter*, Feb. 5, 1952, VI, 24.

Third, the competition is not primarily a price competition but rather one for attracting the most-qualified buyers, that is, the highest quality students. Fourth, the major costs of education, inclusive of extra costs of living and income foregone, are still borne by the individual, though there is a strong public interest involved.

I. *Symptoms of Financial Distress*

It is generally assumed that IHL are in financial difficulties. The Commission also makes this assumption, and lists as factors accounting for the financial problem: inflation, expansion of services, fluctuating student enrollments, needs for enlarged and modernized plant, and uncertain sources of income from gifts, endowment, and government (*Report,* p. 59). We shall return to these factors.

But actually are IHL in distress? Some statistics do not confirm this. Surely, enrollment does not suggest that IHL are in trouble. Fall enrollment in various years was as follows:

Year	*Enrollment*	*Index* (1939 = 100)
1939	1,364,815	100
1943	733,190 (minimum)	54
1949	2,456,841	180
1951	2,116,440	155

Source: *Millett,* p. 67.

It is not generally held that financial difficulties spring from an increased number of customers. (The decline of 14 per cent from 1949 to 1951 is explained by the reduction in "back-log orders.") The Commission is not too helpful in clarifying the relation between enrollment and financial position. Indeed, they point to the fact that "one-third to two-fifths of expenditures may be required for overhead, student personal services, the library, operation of the plant and other activities" (*Report,* p. 80). As the number of students rises, the apportionment of fixed costs over a larger number should bring about a reduction in costs per student; and on top of a 30 per cent rise of enrollment in the 'thirties, the average enrollment in the years 1946-51 exceeded that of 1939 by two-thirds (calculated from *Millett,* p. 67). In fact, Millett shows that, with rising enrollment, costs increase by but 50 per cent of the additional student charges received, and with declines the IHL profit from a reduction of costs of but 50 per cent of student charges foregone.[3] Moreover, it is shown that average expenditures for administration and general purposes were $183 for liberal colleges with enrollment of 500 or less and $118 for those with enrollment of 2,000 or more (the relation of size and costs is an inverse one throughout). "As a general proposition it seems fairly evident that large enrollments are economical as far as expenditures per student are concerned" (*Millett,* p. 175). Moreover, the large rise of enrollment is mainly concentrated on a relatively unchanged number of institutions. Should we leave the junior college out, the increase in numbers of IHL from 1940 to 1950 was but 4 per cent, and with junior colleges included, 12 per cent (*Millett,* p. 103).

[3] Undoubtedly, this oversimplifies the problem. *Cf. Millett,* p. 272.

Hence I conclude that, however difficult planning becomes with uncertain enrollment, the Commission has not proved its point that fluctuations in enrollment account for financial difficulties—unless IHL unwisely expanded capacity irretrievably in response to the veterans' bulge, which generally was assumed to be temporary. (There was some evidence of great pressures felt by *liberal* colleges to increase commitments in response to higher enrollment in the early postwar period.) Perhaps a more accurate way to put this point is that IHL would fare better with stabilized (or better, steadily rising) enrollment, but any financial problems raised by past fluctuations in enrollment have been *more* than offset by the rise in enrollment, accompanied by a reduction in costs per student, the result of economies of scale.

Before leaving this point, I should add the following, lest my position be misunderstood: First, I am not discussing now the problem of inflation, which, of course, raises costs per student in dollars. Second, it is possible that a rise of enrollment of, say, 50 per cent raises problems of new plant and personnel which may put an increased burden on IHL. (I am reminded of the rise of traffic on the railroad which requires a new track.) In the postwar years, however, the construction of new plant was only to a small extent based on expectations of continued enrollments at the peak 1948 level. Third, as student enrollment rises, the contribution of past accumulations of capital or even of current gifts, declines per student. This means that greater recourse has to be had to student fees or public aid. But it also means that for every rise in enrollment, the contribution of additional student income increases relative to costs.

The conclusion is that in the postwar years IHL gained from the rise of enrollment in that costs per student did not rise as much as revenue per student. Where, however, student revenues are small in relation to additional costs per student (*e.g.*, medicine), large losses result from increased enrollment.

What then is the evidence of financial difficulties for IHL? Clearly the rise of enrollment should not raise serious problems, for it is clear that with that rise marginal revenue exceeded marginal costs. Nor do the income statements of IHL suggest serious problems as of today. Thus, in 1950 income was estimated at $2,182 million and expenditures at $2,069 million. The excess of receipts on current account was greater than in 1940 or 1930, though not on a percentage basis. One may ask why, in view of these summary figures, there is so much public discussion of the colleges being in the red. The answer seems to be partly that many private IHL include as receipts only student payments and returns on endowment, with the result that substantial deficits are shown. Gifts, of course, make up the difference. "For fund-raising purposes some college administrators prefer to show the 'deficit' as an encouragement to their campaign for gifts" (*Millett*, pp. 282-83). So far the evidence of distress is unsupportable. But we shall show that IHL face serious problems nevertheless.

Inflation is one of the important causes of financial difficulties. Rising prices hurt, because receipts often do not respond to the rise of costs. According to a crude price index, constructed with the aid of Professor George Stigler, the prices paid by IHL had risen by 76.2 per cent in 1950 vis-à-vis 1940 (*Millett*, p. 116). (This is about 4 percentage points more than the rise in the consumers price index.)

In response to inflation, student fees have not risen *pari passu.* On the basis of figures for enrollment and tuition fees given by Millett, I estimate the rise of tuition by 1950 at 50 per cent, or about two-thirds the rise of prices. Here is a loss to be associated with inflation; for fees are the most important single source of revenue for private institutions, and the second most important for public IHL (*Millett,* p. 297).

This is not, however, a full statement of the situation created by inflation. The Commission devotes much space to the inflation of costs; but in all fairness they should have concentrated equally on the rise of income of the nation. To some extent the rise of income reflects the inflationary process, both the rise of prices and the effects of that rise on output; and to some extent higher income reflects the increase of output associated with gains of employment and productivity not related to inflation. *Nevertheless, the fact is that if inflation is a handicap, the rise of money income is a boon.*

This rise of income largely explains the increase of budgets of IHL from $624 million in 1940 to $2,068 million in 1950 (*Millett,* p. 280). This rise, it should be noted, even exceeded that in national income. Against an increase of prices of 75 per cent, IHL increased their revenue by 230 per cent. Though fees per student rose by substantially less than costs, the rise of enrollment and other income associated in part with the gains of national income made possible the vast income gains of IHL. To present a full picture we should consider both inflation (a cost factor) and rising national income (a receipts factor).

Is there, then, any evidence of financial distress, or at least of inadequate resources? One symptom is the larger part played by tuition in total costs. In private universities, tuition accounted for 46.7 per cent of all educational income; in private liberal arts colleges, 71.7 per cent; in private professional schools, 46.1 per cent. From 1940 to 1950, student fees in private IHL rose from 57.7 per cent of all educational income (adjusted) to 67.8 per cent; and in public IHL, from 22.5 to 29.8 per cent (*Millett,* pp. 292, 300-01).

It is unfortunate that student fees constitute a rising percentage of income; first because, as the Commission observes, this makes IHL too dependent on a source of revenue that varies with enrollment. The effects on competition for students in debasing of standards is obvious. Second because the increase in fees is an antidemocratic element in higher education. (We shall return to this problem later, when we discuss enrollment and alternative methods of solving financial problems.)

Before we leave this aspect of the problem of tuition, we should emphasize one point. The gains of students from a reduced tuition fee in *real* terms over the period of 1940-50 should be contrasted with the increased *real* tuition fee from 1930 to 1940. A comparison of 1930 and 1950 might in many ways be a more appropriate one than that of 1940 to 1950. In fact, from 1913 to postwar, there is much evidence of a substantial rise in the "real" cost of higher education.[4]

A second symptom of financial distress, or at least of inadequate resources,

[4] *Cf.* my *How Shall We Pay for Education* (New York, 1948), pp. 173-81.

is the pay of faculty members. A college is no better than its faculty. Badgered by higher costs for supplies, construction, nonfaculty salaries, etc., the IHL respond by squeezing its faculty. In general, the result is bound to be felt in the quality of men and women drawn into teaching, a result that is furthered also by the increase in work load.[5]

The Commission shows that salaries in IHL have risen much less than in other occupations. Whereas, on the basis of a substantial sample, average academic salaries in public universities were 2.21 times full-time earnings of all employees for the country in 1940, they were but 1.43 times in 1950. This is aside from the gains resulting from reduced unemployment in industry (*Millett*, pp. 274-75). Actually, except in land-grant colleges, pay rose substantially less than the cost of living (50-60 per cent against a rise in the cost of living of 72 per cent).[6] The rise of pay has been small compared to that of other professional groups. This rise of 50-60 per cent should be compared with one of about 150 per cent for physicians, 100 per cent for dentists, of almost 100 per cent for lawyers.[7] Again, we make one reservation: that the reduced economic status of faculty members is greater if comparison is made with 1940 than with 1930.[8]

Should faculty salaries be raised by 25 per cent, the gain over prewar would roughly be 100 per cent; and the average increase would approach that of other professions except for medicine. For the $708 million of departmental instructional outlays in 1950, this would involve an additional outlay of close to $200 million, or more than $1,000 per faculty member (there were 155,000 faculty members in 1947). The $200 million figure also allows for higher salaries in library, extension, and research. This increase of 25 per cent would seem to be a minimum as a condition for fair competition with the other professions and business. It makes no allowance, of course, for any later doses of inflation.[9] I might add that, in 1948, I estimated that an increase of faculty salaries in *private* institutions which would put them in the same relative position as other members of the labor market would require $200 to $225 million additional per year. At that time, faculty salaries *relative* to income in the economy had been reduced by 40 per cent in real terms.[10]

[5] See, for example, *Higher Education for American Democracy: Report of the President's Commission on Higher Education*, hereafter referred to as *PC* (Washington, 1947), IV, pp. 51-52. Here it is noted that teaching burdens are heavier than before the war.

[6] *Millett*, pp. 131-36; *cf.* also S. E. Harris, "Professional Salaries and Tuition, 1947-48," *Bull. Am. Assoc. Univ. Professors*, Spring, 1948, XXXIV, 98-109.

[7] *President's Commission on Financing of Health Services*, Preliminary, Table D-33, (Washington, 1952).

[8] It should be observed that all estimates of average salaries are subject to one important reservation: the proportion of members of different grades or age group varies. Thus, in 1950, there was a much larger proportion of young members and hence the rise was more than it seemed to be; but the percentage rise varied inversely with rank.

[9] For budgetary figures and number of faculty personnel, see *Millett*, p. 118, and *PC*, VI, pp. 38-40.

[10] "The Future of Higher Education in the United States," *Harvard Educational Rev.*, Fall, 1948, XVIII, 202.

Still another indication of financial difficulties is given by an examination of outlays per student:

EXPENDITURES PER RESIDENT STUDENT, 1930, 1940, 1950

	1930	1940	1950
Instructional Expenditures—in Current Dollars			
All Institutions	$217	$189	$300
Educational and General Purposes—in 1940 Dollars			
All Institutions	289	291	277
Universities	291	319	292
Liberal Arts Colleges	284	292	253
Professional Schools	299	277	281
Junior Colleges	230	171	240

Source: *Millett*, pp. 116–140.

In dollars of stable purchasing power, the outlays per student have tended to decline since 1930 and especially since 1940. The reductions are especially large since 1940 for liberal arts colleges. These figures in themselves do not necessarily mean a debasement of standards, for account must be taken of (1) the economies of scale associated with the rise of enrollment and (2) the exploitation of faculty members. Yet it is significant that in a period when real per capita income (even after taxes) had risen by almost 55 per cent, the real outlays per student actually declined. That the number of resident students from 1940 to 1950 per full-time faculty member rose from 11 to 12 and for liberal arts colleges, from 13 to 15, points to less effective teaching (*Millett*, p. 130). The status of the physical plant, to which we now turn, is also relevant.

PLANT OF IHL, VALUE AND COST OF OPERATION, 1930, 1940, 1950

	Plant Value ($ million)			Plant Operation and Maintenance ($ million)	Plant Operation and Maintenance per Resident Student ($)
	All Plant ($ million)	Public[a]	Private[a]		
1930	2,053	882	1,056	57	58
1940	2,754			63	47
1950	4,800	2,642	1,848	201	85

[a] Included here are only universities, liberal arts colleges, professional schools, and teachers colleges.

Source: Calculated and compiled from *Millett*, pp. 165-66, 256-57, and *Report*, pp. 86-87.

The reader will observe that from 1930 to 1950 the rise of plant value has

been more than twice as great for public institutions as for private; that costs of plant operation and maintenance have risen by 2½ times as against a rise of ½ times in cost of operation per resident student and a rise in total value of less than 1½ times.

All conclusions drawn from these figures are subject to serious reservations; for there is little indication of how plant is carried on the books: original cost? replacement value? (The *PC* estimated replacement value at $4 billion in 1947.)[11] Nevertheless, we note that maintenance and operation expenses have risen about three times as much as the cost of consumer goods. The explanation lies partly in the large rise in the costs of the relevant types of labor; in the increase of plant; in the deferred maintenance associated with the war.

The general view seems to be that plant is too old and inadequate for the job to be done. The U. S. Office of Education has estimated construction needs until 1960 at $5 billion; the *PC,* at $8.3 billion (assumption of 4½ million enrollment and re-establishment of prewar space per student); the Commission, $3.6 billion (assumption of 2½ million enrollment and 1948-1949 prices; at present prices, the amount would be in excess of $4 billion).[12]

Even if the needs are put only at $4 billion, it may be concluded that the financial burden would be great. The sum involved seems to be equal to the replacement cost of existing plant; and the outlays per year about 20 per cent of all expenditures for higher education per year. They may well point to serious financial problems for IHL.

Undoubtedly, the financial problems of IHL are serious; and that despite the fact that the Commission has allowed inadequately for the effects of rising incomes. In particular, the reduction of the significance of endowment income and gift income generally (more on this later), the failure of instructional incomes to rise sufficiently to match incomes of competing employments, the failure of real outlays per student to keep up with the gains of the economy, the unfilled needs for construction, the threat to enrollment associated with security—all these point to financial embarrassment, or at least serious problems.

The crucial problem is how can expenses be cut or income increased. On the former, the Commission makes a notable contribution. All kinds of possible economies are suggested and wastes pinpointed (especially *Millett,* pp. 123-30, 167-72; *Report,* 96-99, 105-13).

II. *How to Increase Income*

The major suggestions of the Commission are to obtain more money from business, especially from corporations, and from state governments. It is not easy to pierce this problem because the figures are presented in such a way that the contribution of each source of income is not clear. *Millett* (p. 292) gives a breakdown, not for all institutions, but for each of four groups. From *PC* and Chapters 15 to 19 of *Millett,* I piece together the following:

[11] *PC,* V, p. 18.

[12] *Millett,* pp. 259-63; *PC,* V, pp. 20-21.

PERCENTAGE OF EDUCATIONAL AND GENERAL INCOME FROM SOURCES INDICATED, ALL INSTITUTIONS' COMMISSION ESTIMATE, 1950; *PC* ESTIMATE, 1960*

	Commission Study[a]	*PC* Estimate[b]
Student Fees	29.8	17.3
Endowment	13.9	8.7
Benefactions	6.2	
Federal Government	27.9 (12)[c]	33.0
State and Local Government	55.2 (38)[c]	39.6
	131.0	98.6

[a] Expenses, $1,677 million.
[b] Percentages estimated on basis of 4.6 million enrollment for 1960. Estimated expenses (operating only), $2,587 million.
[c] Corrected figure.
* Source: *Millett,* p. 292 and Chs. 15-19; *PC,* V, pp. 42, 48.

Even if the Federal Government percentage is corrected to allow for the fact that outlays by the Veterans Administration are included in Student Fees, the Commission figure still adds up to 115 per cent. Probably the difference between 115 and 100 per cent is accounted for by the exaggerated allocation to state and local expenditures. (Part of these outlays are not available for educational purposes.) I have therefore put in brackets corrected figures.

As we shall see later, the *PC* estimates understate the contribution of government; for excluded here are proposals for scholarships and capital outlays. Federal outlays then rise to 47 per cent of the $4.3 billion encompassed, and the percentage contributed by the others declines.

A. Student Fees

Let us consider each of the sources of revenues in turn. Although its position is not stated too clearly, the Commission seems opposed to large increases in tuition fees. One reason seems to be that the private institutions are confronted with the competition of public institutions. A second, the unwisdom of depending too much upon an unstable form of income, unstable because of the dependence on enrollment. A third reason is the democratic argument: education must not be made too expensive for the deserving but poor students. The Commission, on the other hand, does point to the much larger proportion of families that now come within the income group able to pay the costs of higher education (*Report,* pp. 133-34).

I wish that the Commission had been bolder in its recommendations. Do not charge according to the cost of the program. Correct. But why not charge according to the gains to be obtained from higher education? That is, on the assumption that these gains are reflected in the later income of the college graduate, why not require that each college graduate and professional school graduate pay a share of the full cost of higher education that will be proportional to his ability to pay in college years and after? In the years of education the poor student would presumably pay nothing, the wealthy student would

pay the full educational costs over four years (say, $1,600), or even somewhat more (see below), and the average student perhaps $800. According to economic conditions and as the IHL gradually accumulate funds from graduates who pay the difference between costs and undergraduate payments while at IHL, the proportion of the $1,600 paid in college years could be reduced. After graduation, each former student would continue to make payments over the earning years, but the size of the individual's payment would be a percentage of his income—the average percentage being so determined that the total sum thus collected from all graduates over a number of years would suffice to pay the total cost of their education, less what they paid as undergraduates and less any scholarship aid made available to the impecunious and able. Each graduate would pay the same percentage with the reservation that there would be a ceiling on the total amount paid. A wealthy alumnus would then be required to pay more than cost, but the excess would be an insurance premium to offset losses resulting from premature deaths, low incomes of some graduates and defaults on the part of others. A student who paid nothing during his college years would of course be required to pay a larger percentage than the student who paid a large part of costs inclusive of the insurance premium during his college years. Thus the individual with an average income during and after college might pay the full cost of his education, those with incomes above the average would pay more, and those with small incomes during college and after would pay little. I should like to see this proposal applied to both public and private institutions, with the result that pressures of state legislators and unfair competition of public institutions would be reduced.

Medical school graduates would be required to pay about $12,000 plus $1,600 for the A.B. (Perhaps $3,000 would be paid during educational years.) Thus, the sum involved in posteducational payments would be about 2 per cent of the lifetime income (about $500,000) of the average physician. Incidentally, the federal government in taxes remitted might well pay one-third of this cost, as the payment may be classified as a gift. (The obligation would be a moral one, and the rate might be increased slightly to cover deaths, failures to pay, etc.)

But let us consider the difference in approach of the *PC* on the one hand and the Commission on the other. It will be recalled from the preceding table that 30 per cent of all income came from student fees in 1950. The contrast in approaches is suggested by an examination of the 1950 distribution of income and proposed income for 1960 under the plans of *PC*, with anticipated enrollment of 4.6 millions. Despite a doubling of enrollment, *PC* calls for students contributing but 17 per cent of the $2.6 billion involved, whereas the actual contributions in 1950 were 30 per cent of $1,677 million. Since the Commission is seeking to raise income by a few hundred million dollars and is concentrating its attention primarily on business and state government as sources of new revenue, the conclusion may be drawn that the proportion to be provided by tuition under the Commission's proposals might fall a little but not rise.

The contribution of tuition depends in part upon the enrollment and in part

upon policy issues. Here, then, is a striking contrast between the Commission and *PC*. Whereas the Commission is inclined to restrict higher education to a limited number, on the grounds that those should have it who can profit by it, *PC* is much more disposed to open the gates wide. Whereas the Commission seems to put the appropriate number at 25 per cent of the college age population, *PC* would welcome 49 per cent through the fourteenth year (sophomore year or junior college) and 32 per cent through advanced liberal or specialized professional years.[13]

The difference in approach toward enrollment is also reflected in student charges. *PC* emphasizes economic disabilities as the explanation of loss of talent and limitation on numbers. Hence the small dependence on tuition. The Commission, basing itself on a valuable study made for it by B. S. Hollinshead (*Who Should Go to College*), draws attention to noneconomic motivation. Of the top 25 per cent on the basis of intelligence tests who might go to IHL, only 40 per cent actually do, and of these 40 per cent only one-half graduate. In other words, but one-fifth of those who should profit from a college education actually attain it (*Report*, p. 49).

B. Government Contributions

Perhaps the main contrast in approach between these two reports is that which appears in their attitude towards contributions from the federal government. It will be noted that, in 1950, contributions of the federal government (exclusive of Veterans Administration) accounted for 12 per cent of the total income of IHL, and that *PC* plans called for 33 per cent. In fact, the total amount was presently to be $850 million (the counterpart of the 33 per cent) and substantial additional amounts for construction, scholarships, and fellowships. *PC* would have the federal government contribute money for scholarships, fellowships, operating expenses, and research. In contrast, the Commission is against any extension of federal aid, not even for scholarships and fellowships.

The members of this Commission seem genuinely fearful of the federal government. Despite the fact that they found no abuses in the British system of subsidies,[14] only one attempt at appointment control under the subsidy program to agricultural colleges (a study for the Commission), fine cooperation in federal grants, a willingness to concentrate research grants on institutions best fitted to carry the research through—despite all of this the Commission warned against any extension of federal aid. These conclusions seem to derive from a fear of federal control, and from an unwillingness to encourage further extension of federal activities.

Undoubtedly, the Commission fears government more than business; for it appeals to business to provide the main part of the few hundred million dollars

[13] *PC*, I, p. 41. *Cf.* my *The Market for College Graduates* (Cambridge, 1949), where I have discussed some of the issues raised by enrollments suggested by *PC*. Unfortunately, the Commission paid scant attention to this important problem.

[14] Studied in a separate volume for the Commission, H. W. Dodd, L. M. Hacker, L. Rodgers, *Government Assistance of Universities in Great Britain*, 1952.

additional required each year. Distrust of the federal government is especially germane in these days of investigations. But, as Hofstadter so well shows, there are also issues involved in the relation of IHL and its alumni and trustees.[15] It is important, as the Commission suggests, not to be dependent excessively on any one interest.

If the Commission is too coy vis-à-vis the federal government, the *PC* would strain the federal budget too much. Even before the Korean War, it would be hard to justify the additional federal outlays for higher education proposed by *PC*. What is more, the implications of *PC*'s proposals are a steady growth of public as against private IHL. In fact, since the funds would be made available primarily to public institutions, the enrollment in public institutions would rise to 80 per cent of the total by 1960, as against little more than 50 per cent in 1950.

Actually, the total contributions proposed for the federal government by *PC* are much greater than is suggested by the table above. In all, the federal government was to provide \$844 million for general expenditures (as indicated by the table), \$217 million for capital, and \$1,000 million for scholarships and fellowships, or \$2,061 million in all. On top of this, state government was to provide \$1,000 million (as indicated in the table) and 433 million for capital. The total of all expenditures for higher education then rises to \$4.3 billion. These amounts are, of course, out of the question at this time and were much beyond the practical even when proposed in 1947.[16]

It is scarcely necessary to add that the proposed burden on state governments would be intolerable. In 1951, total expenditures of state governments amounted to \$15.1 billion.[17] Inflation, large tax drains of the federal government, and the growing distress of local governments excessively dependent on the general property tax have greatly strained state finances. In that year all expenditures of state governments for state institutions of higher education were \$1,166 million. Of this total, \$656 million were spent on current operation other than capital outlays.[18] This figure greatly exceeds the \$445 million made available to public higher education in 1950 for current operation, the total given by *Millett* (p. 325), and hence includes some extraneous items. At any rate, an increase of current outlays from about \$500 million to \$1,000 million and of capital outlays from \$289 (1951 total) to \$433 million would almost double total outlays and raise state outlays for *all* purposes by 4 per cent. An increase of, say, 10 per cent of current and capital outlays (½ of 1 per cent of all outlays), however, is conceivable, and this might yield \$75 million additional.

Both *PC* and the Commission suggested that charges of about 40 per cent

[15] R. Hofstadter and D. C. Hardy, *The Development and Scope of Higher Education in the United States*, 1952 (a Commission volume).

[16] See *PC*, V, pp. 36-42, and VI, p. 49. Also *cf.* my full discussion in "The Future of Higher Education in the United States," *loc. cit.*, pp. 198-207.

[17] U. S. Department of Commerce, *Compendium of State Government Finances in 1951*, p. 8.

[18] *Ibid.*, p. 33.

of all income would overburden state and local governments. Indeed, the Commission recognizes the problems faced by state government, inclusive of the arrogation of major tax revenues by the federal government, the general rise of taxation, the pressure to increase outlays in other directions than education, in part because of the matching of some of these outlays by the federal government (*Report,* pp. 142-48).

C. Gifts and Endowment Income

With increased dependence on government aid ruled out by the Commission (with the exception of what state governments might spare), the only other important alternative for the Commission is greater dependence on gifts. The Commission recognizes that the inflation has wiped out a large part of the endowment. In fact, a combination of increased enrollment, higher prices, and reduced interest rates has reduced the contribution of endowment income per student; and higher taxes and the increased competition of other funds (*e.g.*, community chests, Red Cross, hospital, war) have resulted in a substantial reduction of the share of the Gross National Product going to IHL in the form of gifts in recent years as compared with the proportions received in 1900-1930.[19]

A measure of losses suffered is given by the fact that over a decade, the *value* of endowment in real purchasing power declined by about 50 per cent, or $750 million; and endowment *income* in real terms declined by 50 per cent, despite a rise in dollar amount by $700 million, or one-half. The percentage of educational income obtained through private benefactions was 5.6, 6.6, and 6.2 per cent of educational income in 1930, 1940, and 1950, respectively; but in real purchasing power per student there was a decline from 1940 to 1950 (*Millett,* p. 336). Endowment income as a percentage of *educational* income of private IHL actually declined from 31.2 per cent in 1930 to 25.5 per cent in 1940 and to 13.9 per cent in 1950. The contribution per student in real purchasing power has declined by about 60 per cent (cf. *Millett,* p. 307). The effects would have been much more serious had not the IHL shifted a substanial part of their investments from fixed interest bearing assets to common and preferred stocks and bonds. The resulting gains may, of course, prove illusory should a serious depression or a decline of values ensue. IHL have also profited from the development of a productive program of annual giving. But in general the annual contribution from gifts and endowment income are relatively much less important than they were twenty years ago.

In the light of this background, it is well to consider the major recommendation of the Commission, namely, that business corporations might be asked to contribute $250 million annually to IHL. It will be recalled from our earlier table that the actual contributions from endowment (13.9 per cent) and benefactions (6.2 per cent) were 20.1 per cent as against the contribution of 9 per cent from these two sources proposed by *PC* for 1960.

In 1950, the corporations contributed about $250 million in gifts of all

[19] For a discussion of some of these issues, see my *How Shall We Pay for Education?* (New York, Harper & Brothers, 1948), pp. 133-61.

kinds, of which IHL received 17 per cent, or about $45 million. All gifts were about 0.61 per cent of corporate income before taxes (*Report*, pp. 168-69). The actual cost to the corporation and the stockholders (assuming *ultimate* distribution of all income and unchanged taxes) was about 0.15 per cent of corporate income, with the federal government contributing the remainder through tax remissions. Additional gifts to IHL of $250 million would, therefore, involve a cost to the corporations and their owners of but $60 million, or roughly 1/10 of 1 per cent of corporate income before tax. The case for increased gifts is indeed strong, especially when consideration is paid to the direct and indirect contributions of IHL to the income of corporations.

But there are obstacles. Although corporate giving has increased by ten times since 1936, this rise is relatively little more than the gain in corporate income. Legal obstacles are important, though these are gradually being overcome. The experience of the medical schools in a well organized campaign is significant. This group raised but $3 million *in all* over several years, whereas the additional funds required by medical schools probably exceed $40 million per year.

Finally, the question as to the price to be paid has to be considered. In response to the plea made by the Commission, *Barron's*, in a lead editorial Jan. 1, 1953,[20] has replied to IHL that their financial trials are of their own doing; that their professors are responsible for full employment policies, inflation, higher taxes and controls; and that, if they want to prove themselves worthy, they had better dismiss their leftist professors. The editorial in *Barron's* does not, I hope, represent the general attitude of business. In fact, as President Conant said in his 1952 report, the *great* business benefactors have not set conditions. But it is also clear that as IHL seek for more funds from special interests, they may increasingly be pressed to yield control over educational policies.

Conclusion

IHL are in need of additional resources. The case for increased income rests on the failure to improve the quality of higher education in proportion to the over-all rise of material resources and standard of living; on the large relative deterioration of the economic status of the faculty; on the need of not relying excessively on tuition paid during school years; on the effects of higher taxation, inflation, lower yield on capital, and the rise of enrollment on the contribution of endowment and gift income per student.

The crucial issue is where to obtain additional resources. Whereas the Commission, fearing the federal government, urges greater dependence upon business and continued high tuition from students, the *PC*, impressed by the relation of economic resources to attendance at IHL and sympathetic to the extension of government aid, would rely on the federal government primarily and state government secondarily. The difference in approach rests partly on the fact that the Commission weights more heavily than the *PC* the noneconomic factors accounting for nonattendance at IHL, and also is more

[20] *Cf.* my reply, Jan. 29, 1953.

disposed to deny entry to IHL to those who might only depress standards. Those who are fearful of interference by both government and business, may find my suggestions for financing higher education congenial. This proposal would require the student to pay the entire cost of his education over his college and working life.

May I end by saying that the Report and the supplementary volumes are the most important study of the economics of higher education yet made available. No one interested in these problems can afford to neglect these volumes.

COMMUNICATIONS

Elementary Algebra and the Equation of Exchange

During the six years since the close of World War II, twenty textbooks in the field of money and banking or money and business activity by American economists have been published.[1] With one exception, these books refer to the equation of exchange in discussions of the interrelationships of such concepts as the quantity of money, its rate of use, the level of prices, the quantity of goods and services sold, and the money volume of transactions. In all nineteen books, the equation is written in the traditional form, MV = PT, or in an equivalent form.[2] This appears to be an algebraic equation of the type $xy = wz$, or $x = \frac{wz}{y}$. However, if a reader attempts to treat the equation of exchange, with the definitions of the terms as they are given in these books, as an algebraic equation, and to substitute true numerical values (or approximations to such values), various difficulties turn up which ought not to appear when using a simple algebraic equation.

In two of the nineteen books it is pointed out that the term PT is a substitution for the term Σpq. However, in both cases the equation is developed in a form which attempts to add nonhomogeneous quantities, or measure a "meaningless heap," to use an expression of the late Professor Schumpeter.[3] Of the seventeen books in which the expression PT, but not Σpq, is used, two define P as an index number with an unspecified base, while treating the other three elements in the equation as absolute quantities. In two books, both P and T are defined as index numbers, with M and V absolute quantities. Any attempt to insert numerical values in the equation MV = PT by using these definitions and index numbers with a base of 1 or 100 (which are the only bases in common use) gives the absurd result of a figure of several hundred billions being equal to a figure of a few tens of thousands, or smaller.

[1] Books by persons from other countries now teaching in the United States are included. A list of the books may be obtained from the author.

[2] By "equivalent" form I mean a form which differs from MV = PT only by the use of other symbols for one or more of these four letters, or by the shifting of one or more of these four elements to the other side of the equation by using its reciprocal.

Some of the authors retain Fisher's distinction between currency and its velocity (MV) and bank deposits and their velocity (M'V'). This distinction will be ignored here; that is, M refers to all forms of money used as circulating medium, and V to the average velocity (number of uses per time period) for all transactions, or for purchase of final products.

[3] Joseph A. Schumpeter, *Business Cycles* (New York, McGraw-Hill, 1939), p. 484. The general concept of an assorted lot, or heap, of goods and services does, of course, have meaning, and so do the concepts of larger or smaller applied to two or more such heaps. However, quantitative measurement of such a heap, for use in mathematical formula, is without meaning unless a common unit of measurement, applicable to all the items in the heap, is specified.

To avoid a fallacious result, one of the books specifies that P, the index number of the general price level, represents the average price paid for the various goods and services, and that T is an index number of quantities with the unit of measurement of each class of goods a dollar's worth of that class in a base year. Two other books which do not refer to P and T as index numbers use the same procedure. While this procedure works out correctly, it is impossible to apply to factual data because it would require complete coverage and a knowledge, for every item, of the physical quantity which sold for a dollar in the base year. The basic methodology of index numbers, that of sampling, weighting, and expression of the results in the form of relatives to 100 for the base year, cannot be used; and the results of the procedure cannot properly be called index numbers.

Of the remaining books, one has no definition nor description of P and T, merely stating that the PT factor is the all-inclusive sum of money transactions in the period and may be subdivided into its parts. In all the others the phrases used in defining both P and T are ambiguous, and some of them are subject to the "meaningless heap" censure. In none of these cases would it be possible to find numerical values to fit the definitions without additional specifications, *i.e.*, without altering the definition.

In thirteen of the books, the equation, MV = PT, is described as a "truism" or "identity." With the ambiguous or inconsistent definitions used, this is not necessarily true: some of them, in fact, lead to false and absurd results when applied to numerical data. Moreover, only two of the books point out that for any time period payments for things (MV) are not precisely equal to the sum of the values of those sold (Σpq), because of debts carried over the ends of the period.

The reason for most of the inconsistent and inadequate definitions of the terms of the equation of exchange appears to be the general acceptance of the expression PT for Σpq. As indicated above, the latter term is not used in seventeen of the textbooks. Of the two authors who use both expressions, neither points out the important fact that the latter term cannot accurately be represented by the former. This obvious fact had been mentioned by at least one previous author of a money and banking textbook, Harold Reed. "Technically, of course PT is bad mathematical terminology. T refers, usually, to many commodities, to cloth, to corporate securities, to iron, to house rents, to labor. There is no homogeneous unit of T. It is impossible to add coal to cloth to corporate securities to iron to rents to labor."[4] Schumpeter had also emphasized the ambiguity and lack of meaning of expressions such as "the price level" except when defined in terms of the relative magnitude of Σpq for one period compared with another period, with an unchanged collection of goods and services.[5] Inasmuch as Reed's book was published four years earlier, and Schumpeter's seven, than any of the nineteen postwar texts on money and banking, it would seem that later writers might have taken a cue from them and avoided the ambiguous and inaccurate terminology.

[4] Harold L. Reed, *Money, Currency and Banking* (New York, McGraw-Hill, 1942), p. 144.

[5] Schumpeter, *op. cit.*, pp. 451-56.

All the authors use the equation of exchange in discussing changes in the value of money, or level of prices, over time. This implies an application of the equation of exchange to successive time periods. However, none of the authors develops the equation in a suitable form for this purpose, though this is easily done with very simple algebraic manipulations, starting with the correct form of the equation for a particular time period, $mv = \Sigma pq$.[6] The procedure is to use subscripts, o for the base year and i for later years ($i = 1, 2, \ldots$); then, since both sides of an equation can be divided by the same quantity (represented by either side of another equation), to write

$$\frac{m_i v_i}{m_0 v_0} = \frac{\sum p_i q_i}{\sum p_0 q_0}.$$

In this form both sides of the equation represent the relative aggregate value in the year i, in comparison with the base year, of the transactions to which the equation is applied. The money-velocity side of this equation can be rewritten in the equivalent form $\left(\frac{m_i}{m_0}\right)\left(\frac{v_i}{v_0}\right)$, *i.e.*, as simple relatives of the quantity and velocity of money. On the price-quantity side of the equation, we can multiply both numerator and denominator by the same quantity, selecting either $\Sigma p_i q_0$ or $\Sigma p_0 q_i$. We then have

$$\frac{\sum p_i q_i}{\sum p_0 q_0} \cdot \frac{\sum p_i q_0}{\sum p_i q_0} = \left(\frac{\sum p_i q_0}{\sum p_0 q_0}\right)\left(\frac{\sum p_i q_i}{\sum p_i q_0}\right),$$

which is the Laspeyres formula for a price index with a Paasche formula for a quantity of goods and services index; or

$$\frac{\sum p_i q_i}{\sum p_0 q_0} \cdot \frac{\sum p_0 q_i}{\sum p_0 q_i} = \left(\frac{\sum p_i q_i}{\sum p_0 q_i}\right)\left(\frac{\sum p_0 q_i}{\sum p_0 q_0}\right),$$

which is the Paasche formula for a price index and the Laspeyres formula for the quantity index. If we do not like the combination of a price index which uses weights of the base year and a quantity index which uses weights of the current year, or the reverse combination, we can substitute for both indexes the formula recommended by Bowley more than a half-century ago, and later called by Fisher the "ideal" formula.[7] Other formulas may also be used. In each case the algebraic manipulations by which the chosen price index formula is introduced into the equation will provide a corresponding formula for the quantity index.[8]

[6] Lower-case letters are used here for the money-velocity side of the equation, as well as the price-quantity side, so that the capital letters can be used for the index number form to be developed.

[7] The simplest way of showing that this substitution can be made is to make it and then simplify the result, which will reduce to the original $\frac{\Sigma p_i q_i}{\Sigma p_o q_o}$.

[8] For an excellent brief discussion of various index number formulas and their origins see the article, "Index Numbers," by C. M. Walsh, in *Encyclopaedia of the Social Sciences* (New York, Macmillan, 1932), VII, 652-58.

With the foregoing we can write $MV = PT$, defining all four of the terms as indexes to the same base, with M and V ordinary relatives and P and T index numbers based on the "ideal" formula or on some other selected pair of formulas. For practical application, price and quantity index numbers based on formulas which are not properly paired with each other, mathematically, may be used as approximations; even those derived by use of the "ideal" formula will also be approximations because of computation from samples rather than the whole universe of transactions to which the equation is applied.

There is another form of the equation of exchange, closely related to the foregoing, which is also of great use in dealing with the central problem of monetary economics, namely, the relation of the price level and monetary policy to business fluctuations. This form may conveniently be written $M_tV_t = P_tT_t$, with the terms defined as trend-adjusted index numbers, or percentage ratios to trend, and with the trends appropriately selected and interrelated. Selection of the trends is as follows: for T, the average rate of growth in output or physical volume of transactions over a substantial period of time (several "business cycles"); for V, the reciprocal of the average rate of growth (positive or negative) in holdings of cash balances (money) relative to aggregate expenditures for the type of goods and services included in T, also over a period of several "business cycles"; for P, none, on the ground that neither a falling nor rising price level is characteristic of a stable economy; and for M, the combined average rate of growth in output and in cash holdings relative to expenditures, on the ground that this is the rate of growth in the quantity of money needed for maintenance of economic stability, or maximum production, minimum unemployment, and a stable level of prices.

In view of the usefulness and simplicity of derivation of the index number and trend-adjusted index number forms of the equation of exchange, it is difficult to understand why monetary economists and economic theorists generally have neglected to make use of them, or why all the textbook writers either distort the Fisherine form of the equation by use of ambiguous or inconsistent definitions, or use definitions which are too clumsy or cumbersome for application to statistical data. It seems incredible that the appearance in American economic literature of the index number form of the equation, and its derivation, should have been delayed for several decades after Fisher's popularization of the equation, and that only one attempt has been made to utilize the equation in a practical way by developing a set of index numbers of all four of its elements.[9]

CLARK WARBURTON*

* The author is an economist in the Division of Research and Statistics, Federal Deposit Insurance Corporation. The opinions expressed are personal only.

[9] The earliest presentation of the index number form of the equation of exchange and its derivation with which the writer is acquainted is in a paper, "Comments on the Equation of Exchange," given at the Fifth Annual Conference, Indian Economic Association, Patna, India, December 1922 (*Proceedings* of the Conference, pp. 88-105). More detailed treatments of the relation of price and quantity index numbers to the theory of the value of money and the equation of exchange are given in François Divisia, *Economique Rationelle* (Paris, Gaston Doin, 1928), Ch. XIV, and in Griffith C. Evans, *Mathematical*

A Note on the Relationships of Bank Capital to the Lending Ability of the Commercial Banks

There has been published in recent months a spate of articles, pamphlets and monographs which have discussed the problems of bank capital.[1] Most of these take note of the very low ratio of capital accounts to assets or deposit liabilities.[2] In the samples which have come to my attention this is universally regarded as an unfavorable development from the standpoint of the longer-term interests of the bankers, their customers and patrons. The authors point with alarm to the fact that inadequate bank capital creates a condition inimical to the maintenance of depositor confidence, to the ability of the banks to absorb losses and still maintain their solvency—and lastly, that inadequate bank capital curtails the ability of the banks to supply their business customers with needed credit.

It is with this last "function" of bank capital that issue is here taken. Judged by the many variants of statements concerning the relationships of bank capital to the lending ability of the banks, the belief is apparently widely held that the capital accounts constitute a *source* of loan-funds; that commercial banks loan out the savings of the banks' owners.

Such statements are common and are to be expected in the trade journals, but when they appear in the more scholarly publications, or are authored by professional economists, they should not be allowed to go unchallenged. Four such statements have been selected for examination here.[3]

Introduction to Economics (New York, McGraw-Hill, 1930), Ch. IX. Index numbers of the elements of the equation of exchange for the United States for the period 1919-1947, quarterly and annually, are given in a paper, "Index Numbers of the Elements of the Equation of Exchange," presented by the writer at a joint meeting of the Econometric Society and the American Statistical Association, December 28, 1948 (available in mimeographed form; abstract published in *Econometrica,* Apr. 1949, XVII, 176).

[1] The terms capital and capital accounts are used synonymously. As so used they include the aggregate of capital stock, surplus, undivided profits and all net worth reserves.

[2] Bank capital as a percentage of total liabilities has declined, with minor reversals of the trend, for the past seventy-five years; from a percentage of approximately 35 in 1875 to about 7 as of the present time. The series is characterized by a number of sharp declines in the capital ratios with connected plateaus of relative stability. In the 1880's, for example the level was about 30 per cent as compared with about 20 per cent during the 1900-1910 period, and about 12 per cent in the 1920's. In the course of World War II, the ratio fell decisively below the 10 per cent level, traditionally regarded as a minimum for safety. Since the end of the war the capital ratios have tended upward, largely due to an expansion of bank capital accounts. This in turn is almost entirely attributable to retention of earnings.

[3] *The Increased Dependence of Business on the Commercial Banks,* Bull. No. 175, Institute of International Finance, The Schools of Business of New York University, Mar. 4, 1952, pp. 4, 5, 12, 19. Gaylord A. Freeman, Jr., *The Problems of Adequate Bank Capital,* Illinois Bankers Association, May 23, 1952, pp. 2, 5, 10, 13, 44, 47, 79, 80. Other examples could be cited from an article by Marcus Nadler, "The Trend of Commercial Loans," *Finance,* Jan. 15, 1952, see especially pp. 41, 42, 95. Similar misconceptions of the basic nature and distinctive character of the commercial bank as contrasted with all other types of financial institutions are to be found in many of our recent money and banking textbooks. See, for example, George W. Woodworth, *The Monetary and Banking System* (New York, 1950), p. 84, and Chart 70; E. W. Boehmler, R. I. Robinson, F. H. Gane and L. C.

On page 12 of the bulletin published by the Institute of International Finance is the statement:

> If the need for working capital should grow it will create problems for the commercial banks, particularly for those whose capital is small in relation to deposits and risk assets. . . . Therefore, unless the banks take measures to expand their lending ability by increasing their capital resources. . . .

And at the end of the bulletin (pp. 18, 19, italics added):

> In view of the vital function banks perform in the economy and *the definite relationship between the capital resources of a bank and its ability to lend* it is important that banks be in a position to increase their capital resources either through retained earnings or the sale of stock in the open market. In this way the banks would be enabled to meet the increased requirements of industry and trade.

Similar notions are to be found in the monograph published under the auspices of the Illinois Bankers Association. On page 5, footnote 12, of this publication the author states:

> . . . if there is a continued growth of demand for credit the limited capital of many banks will prevent their providing the additional credit.

And on page 79:

> The principal function of capital is to . . . enable the bank to continue in business, and provide needed credit.

It is recognized of course that there are some relationships between bank capital and the ability of the banks to lend. This note is merely a denial that bank capital is a *source* of such funds.

State and national laws have long placed limitations on the amount any single bank can lend to any single borrower. Most of these limitations have defined these maximums in terms of a percentage of capital or of capital and surplus. However, such a limitation does not curtail the ability of the entire system of banks to make loans, except in a purely administrative sense.

Furthermore, supervisory authorities have tried, with virtually no success, to relate the volume of bank capital to the volume of liabilities, or of risk assets. To the extent that low capital ratios cause bank examiners to put pressure upon bankers to restrict their lending, it may be said that "inadequate" capital can lead to a denial of business loan applications. But the record shows that various "rules-of-thumb," including the venerated "10 per cent" criterion, have long since gone by the board. Unless supervisory authorities are much more successful in the future than they have been in the past in restricting bank lending to a definite ratio of capital funds it could not be concluded that bank credit is definitely tied to or limited by the volume of bank capital. Indeed if current trends are indicative, it seems probable that supervisory au-

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Farwell, *Financial Institutions* (Homewood, 1951), p. 91; Jay L. O'Hara, *Money and Banking* (New York, 1948), p. 101; and Charles L. Prather, *Money and Banking*, 5th ed. (Homewood, 1953), p. 223.

thorities will put even less credence in "rules-of-thumb" and more reliance on qualitative judgments in determinations of the adequacy of bank capital.[4]

The foregoing should not be interpreted, however, as a denial that the expansion of capital accounts will not ordinarily increase the lending ability of the banking system, *but the increased lending ability derives from the fact that an expansion of the capital accounts, by causing a diminution of deposits in the system, releases excess legal reserves.*

While it is possible that capital accounts of banks could grow out of the investing of previously idle currency hoards, or from gold flights from other countries where the owners chose to invest their funds in new bank stock, these and other similar sources are extremely unimportant. For all practical purposes the only really significant "outside" source of capital for banks is the bank-held savings of bank customers. Consequently an expansion of commercial bank capital accounts tends to cause an equal reduction in the banks' deposit liabilities. The increased lending ability of the banking system therefore grows out of the release of excess reserves within the system.[5]

An expansion of the capital accounts of banks through the retention of earnings will increase the lending ability of the banks for the same reason since the retention of earnings tends to cause demand deposits to decline by the same amount that the equity accounts (undivided profits) are expanded. Thus excess reserves are released and on the basis of these the banks can, if they have the opportunity, create a structure of loan-deposits equal to the volume of earnings retained.

[4] Neither the supervisory staff of the Comptroller of the Currency nor that of the Federal Deposit Insurance Corporation adhere at present to any mechanical or fixed standard or ratio in gauging the adequacy of bank capital structures. Their practices, which are borne out by various public statements, indicate that they believe that a uniform standard would be applicable to only a few banks, and that the test of capital adequacy for any given bank should give consideration to the quality and nature of its assets, the quality of its management, and the general manner in which the business of the bank is conducted. See statement by Maple T. Harl, chairman, Federal Deposit Insurance Corporation, "What Are Proper Standards For Bank Capital?", *Banking*, Jan., 1952, xliv, 35. See also the *Annual Reports* of the Comptroller of the Currency and the Federal Deposit Insurance Corporation. For a summary of views and practices of state supervisory authorities see: *A Report of the Committee on Risk Asset Ratio Study*, New York State Bankers Association, Mar., 1952, especially pp. 17-22.

[5] These effects could easily be overestimated. If, for example, all holders of time deposits in commercial banks could be induced to buy new issues of bank stock this would not increase the lending ability of the banks by the volume of these deposits, but only a fraction thereof. Taking the 1952 year-end figure of approximately $40.3 billion for the total volume of time deposits in all commercial banks and assuming an average reserve ratio of .05 required against these deposits we obtain a figure of $2.0 billion as being the volume of excess reserves released if all time deposits were invested in new bank stock. On the basis of these excess reserves the banks could (*cet. par.*, including the public's holdings of currency and assuming an average reserve ratio of .15) create approximately $13.3 billion in new loan-deposits. The formula for the above calculation is $L = C/R$, where L represents the volume of loan-deposits potentially creatable; C, the volume of excess reserves in the system; and R, the weighted arithmetic average of the reserve ratios applicable to demand deposits for all banks in the system.

It is impossible to make a similar estimate for demand deposits since the volume of these deposits held idle and regarded by their owners as savings is indeterminate.

Substantially the same reasoning applies to any given bank in the system. If the expansion of the capital accounts comes through the retention of earnings, the lending ability of the bank is increased because there has been a decline in the deposits of the given bank and/or other banks, thus causing an expansion of the bank's excess reserves. If the retention of earnings is at the expense of the customers of other banks, the bank's lending ability will be much more enhanced than were these earnings at the expense of the bank's own customers. Income derived from the customers of other banks adds to the total as well as the excess reserves of the bank, while earnings derived from the bank's own customers add nothing to the bank's total reserves. In the latter instance excess reserves are increased only a fractional part (around 15 per cent on the average) of such earnings.

If the capital accounts of a given bank are expanded through the sale of new issues of stock this will also cause a reduction in the deposits of the given bank and/or other banks, unless (which is unlikely) the stock is purchased out of currency hoards. The lending ability of the bank is again increased for the same reasons given above; viz., the release of excess reserves (where the owners of the new stock are customers of the given bank), or the expansion of total (and excess) legal reserves (where the owners of the new stock are customers of other banks).

Whereas the lending ability of a bank is increased more by the sale of bank stock to (or the derivation of earnings from) the customers of other banks, this advantage is at the expense of other banks with the result that, from the standpoint of the system, the lending ability of the banks is increased only to the extent (or less) that deposits are utilized (and thereby destroyed) in these operations. Even if it is assumed that the earnings retained, or the savings utilized in the purchase of new issues of stock, are entirely at the expense of the demand deposit, rather than the time deposit, holdings of bank customers, new loan applications can only be accommodated to the amount of these deposits.

Lest this equivalence be misinterpreted it is perhaps well to reiterate the well-established fact that commercial banks, in their lending or investing activities, create credit. And from this it has been repeatedly affirmed that the commercial banks do not, indeed cannot, lend out the savings of their depositors. From the foregoing discussion it should also be apparent that the commercial banks cannot loan out the savings of their stockholders, either voluntary or forced. But there is this difference: When a bank depositor saves and keeps his savings in the bank this simply results per se in *idle* deposits, but no change in either the volume or the composition of the assets and liabilities of the banks.[6] There is thus no change in the lending ability of the banks unless the depositor chooses to transfer these savings from the demand to the time category, or from a bank having a higher to a bank having a lower reserve ratio requirement.

[6] This action on the part of bank customers by reducing the turnover of funds dampens effective demands in the markets, and thereby tends to diminish the future opportunities of the banks to lend.

When a bank customer saves and puts these savings into new bank stock, or a bank stockholder has savings forced upon him by action of the board of directors, this causes a diminution of deposit liabilities and a corresponding increase in the banks' net worth accounts. Since reserves are only required against deposit liabilities this creates excess reserves in the system and consequently increases the lending ability of the banks. If the banks are able to take advantage of their new-found excess reserves they do so by accommodating borrowers (with immaterial exceptions) with newly created deposits. Thus it may be said that, in so far as borrowers are accommodated with deposits, the commercial banks originate all funds lent. They are not middlemen in the lending process for either depositors or stockholders.

LELAND J. PRITCHARD*

* The author is professor of finance at the University of Kansas.

Wicksell on Fiscal Reform: Further Comment

With its emphasis on Wicksell's benefit-*theory* of tax reform, Professor Buchanan's recent "Comment"[1] supplements what I had to say mainly about Wicksell's *practical program* of fiscal reform in my article.[2] Buchanan's discussion is an excellent summary of Wicksell's theory of "justice in taxation" as expressed in *Finanztheoretische Untersuchungen*. This summary, however, needs amplification on an important point because Buchanan neglected to mention the preconditions Wicksell regarded as necessary, and the existence of which he postulated in developing this argument, for his schema for "just taxation." These conditions were:

1. Intensively developed, democratic political institutions involving *inter alia* universal adult suffrage and elections on the basis of proportional representation, to the end that all minorities might achieve political representation, in proportion to their strength.[3]

2. An undertaking by parliament always to consider proposed expenditures along with the taxes necessary to support them and to allow minorities ample opportunity to veto and amend majority proposals which would not pass unless they received a qualified majority vote (of at least two-thirds, or more, if possible and practicable) of the members of parliament.

3. *A revision by gradual confiscatory taxation of some existing property relations and of the distribution of income and wealth so that the revised distribution might be sanctioned as "just" by a social consensus reflecting the contemporary sense of justice.* Among the property rights, types of wealth, and income he would have modified and subjected to confiscatory taxation were: (a) the then unlimited right to acquire property by inheritance, (b) monopoly gains of all sorts, (c) most varieties of "unearned" incomes and increments in asset values—particularly in land values in communities with increasing den-

[1] James M. Buchanan "Wicksell on Fiscal Reform—A Comment," *Am. Econ. Rev.*, Sept. 1952, XLII, 599 ff.

[2] Carl G. Uhr "Knut Wicksell—A Centennial Evaluation," *Am. Econ. Rev.*, Dec. 1951, XLI, 829 ff.

[3] Knut Wicksell, *Finanztheoretische Untersuchungen* (Jena, 1896), pp. 123 ff., and also see one of his tracts *Rösträtten och Skatterna* (The Franchise and Taxation), (Stockholm, 1898).

sity of population, (d) value-gains that accrue, and abnormal concentrations of wealth that are frequently made possible for large property owners and entrepreneurs at the expense of small owners and of the propertyless, in the course of business fluctuations and as a result of sustained periods of inflation, such as occurred during the first World War and its aftermath.[4]

Given these conditions—and it is clear that with respect to condition (3) this was demanding a good deal—Wicksell was anxious to place all decisions concerning public expenditures and taxes (except taxes to cover interest and amortization on the public debt, for reasons given on page 600 and note 6 of Buchanan's "Comment") under the voluntarist principles of relative unanimity of consent by all parties in a thoroughly representative parliament. Because these preconditions were not even approximately realized during his lifetime, Wicksell found it necessary to agitate for political reforms to make them possible, and to support progressive income and property taxes. Although the latter were not a perfect answer to his quest for justice in taxation, they tended at least to remove the worst injustices perpetrated under the tax systems of his day. Those systems relied almost exclusively on indirect taxes enacted by parliamentarian majorities representing mainly the propertied classes because, until the close of World War I, the franchise was generally denied to the propertyless.

Wicksell's advocacy of the "confiscation principle" of taxation to obtain the desired revision of the distribution of wealth, was mainly intended (a) to eliminate all forms of rent from the category of private income and (b) to make restitution for and prevent the recurrence of the "capitalistic bolshevism"[5] that affects the distribution of wealth when the value of money is undergoing substantial change, especially during periods of inflation.

To the extent possible, Wicksell wanted to eliminate rent from private income by taxation, because income of this type can presumably be taken from individuals by the state without impairing their production incentives and without reducing the output of goods and services. Then the state in turn ought to use the corresponding revenues for a secondary distribution of social real income to all citizens in the form of an increasing variety and quantity of useful public or social services, especially in the fields of education and public health, for expansion of these services would provide greater equality of opportunity for all citizens, and they would also contribute both directly and indirectly to an increase in general productivity.

In order to reinforce the foregoing long-run aim by appropriate short-run action, Wicksell held that the pervasive changes in the distribution of wealth which are a byproduct of significant changes in the value of money should, so far as possible, be prevented. If they could not be prevented entirely, then

[4] Knut Wicksell, *Finanztheoretische Untersuchungen*, pp. 143-50.

[5] This descriptive phrase was coined by Wicksell's colleague, D. Davidson, in an article on "Valutaproblemets teoretiska inneböird" (Theoretical Implications of Problems Relating to the Value of Money), *Ekon. Tids.*, 1920, xxii, 83 ff. It referred to the progressive acquisition of wealth by the debtor class or enterpreneurs from creditors and rentiers that occurs in an inflationary period as a result of the decline in purchasing power of money. The more protracted the inflation, and the higher it spirals, the more thorough this form of "bolshevism."

upon the return of more stable monetary conditions, arrangements should be made for partial restitution of losses[6] to those who had been impoverished as a result of the preceding inflation (or deflation). To his mind, a money of stable value, *i.e.*, a nearly constant price level, was a prerequisite for rational economic conduct, especially with respect to individuals' plans or investments for the future. Moreover, this stability seemed essential to Wicksell for the protection and progress of democratic political institutions in society.

It is, of course, possible that Buchanan may be right in saying that because of the existence of strong, functional groups and because of the Keynesian impact on fiscal policy, Wicksell's voluntarist benefit-taxation-schema is "deserving of more careful consideration than when . . . first proposed."[7] Yet this seems rather improbable. A proposal that presupposes a "just distribution" is not much more applicable to our own than to Wicksell's world, not unless it can be shown that Wicksell was wrong about this precondition and that, in fact, his schema is applicable with net advantages regardless of the character of society's distribution of income and wealth. Thus far, this has *not* been demonstrated. Further, the contemporary fiscal problem also involves more variables than those Wicksell recognized in *Finanztheoretische Untersuchungen* (though he came closer to seeing most of them in his monetary analysis); hence its solution apparently requires a different approach and treatment from that which it received there.

CARL G. UHR*

[6] Wicksell's most explicit argument for the restitution scheme is found in the separate statement he made in a report to the Swedish government in 1920 as a participant in its Commission of Financial Experts, *"Bilaga till Utlåtande av 1920 Års Finanssakkunnige"* (Appendix to the Report of the 1920 Commission of Financial Experts), (Stockholm, 1920).

[7] J. M. Buchanan, *loc. cit.*, p. 602.

* The author is an economic consultant in San Francisco.

Domestic Air Line Self-Sufficiency: Comment

Mr. Harold D. Koontz, in two recent articles[1] has advocated the merger of the present sixteen trunk line air carriers into a system of six carriers.[2] That consolidation is essentially to be in the form of combining the twelve small carriers into two systems, while leaving the four largest carriers little changed.[3] Feeder lines are not considered.[4] The purpose of this note will be to analyze the argument on which Koontz bases his policy recommendation, rather than to examine that policy and the alternatives, if any, to it.

Koontz begins with an attempt to estimate the amount of the subsidy. He

[1] "Domestic Airline Self-Sufficiency," *Am. Econ. Rev.*, Mar. 1952, XLII, 103-25; and "Economic and Managerial Factors Underlying Subsidy Needs of Domestic Trunk Line Carriers," *Jour. Air Law and Commerce*, Spring 1951, XVIII, 127-56.

[2] *Review*, p. 124.

[3] *Journal*, p. 154.

[4] There are some 20 feeder or local service lines, the largest of which are about the size of the smallest trunk lines, measured in gross revenue. The classification is arbitrary; essentially those in operation prior to 1938 are classified as trunk lines, while feeders have entered the industry since the end of the war.

assumes that a service rate (*i.e.*, nonsubsidy rate) for the carriage of air mail would be the same as for the carriage of passengers.[5] It is difficult to see the reason for assuming that a mail rate yielding the same revenue per ton mile as the passenger rate is thereby somehow a nonsubsidy rate. The passenger service is sold in a mass market, in which, although there is often but one seller of the air line service, there is a broad group of buyers who choose air service as only one of several competing means of transport. In such a market, conventional pricing concepts are applicable. Mail, however, is transported under conditions approaching those of bilateral monopoly, with a single buyer of the service, the Post Office, and often a single seller, the air line serving the pair of cities involved. Nor is there even any relation between the postage rates which Congress establishes for the Post Office to charge the users of the air mail service and the rates which the Civil Aeronautics Board establishes for the Post Office to pay for the carriage of its mail. The price elasticity of demand for the carriage of air mail may well be zero. It is hard for me to see why passenger rates, an average of coach and first-class fares in continually variable proportions, even assuming that existing passenger fares represent optimum pricing in the various passenger markets being served, should somehow represent a norm for mail rates, which would appear to be a completely different market. In a bilateral monopoly situation, such as the relation between the Post Office and the air lines, there will be no market price in the usual sense, and price will be determined by bargaining. If subsidy is defined as payments in excess of market price, the subsidies are also indeterminate. As Locklin has argued, it is only in a special case that the Ernst and Ernst method is appropriate.[6]

Koontz shows that if the air lines had been paid for the carriage of mail on the basis he suggests, all lines would have been paid less, with a resultant decrease in net operating income.[7] In 1949 only the big four carriers would have

[5] *Review*, p. 105; *Journal*, pp. 128-29. Although Koontz sets forth no justification for his assumption, that assumption is perhaps derived from the report of Ernst and Ernst on the separation of compensatory mail pay from total mail payments ("Report of Ernst and Ernst on Survey of Separation of Compensatory Mail Pay from Total Mail Payments to Domestic Airlines," *Journal*, Spring 1951, XVIII, pp. 206 ff.) They write, "Since passenger and mail are the primary services, and are rendered jointly (passengers contributing more than 85 per cent of the total revenue and mail approximately 6 per cent), we believe that the compensation paid for both services should be related, varied only to cover the cost differential existing because of the exclusive passenger or exclusive mail costs involved" (*ibid*, p. 233). This represents, however, a statement of faith on the part of the cost accounting firm, and does not necessarily constitute a principle to be accepted without economic analysis.

[6] D. Philip Locklin, "A Critique of Proposals to Separate Subsidy from Air Mail Pay," *Jour. Air Law and Commerce*, Spring 1951, XVIII, 166-180.

That case requires that total air line revenues, including mail revenues related to passenger fares, be just sufficient to cover all costs including a fair but not excessive return on investment. Under conditions in which total revenues, including mail revenues so fixed, are greater or less than total costs, including returns on capital, the establishment of mail rates based on passenger fares is not proper, and the fixing of mail rates above the level of fully allocated costs based on market consideration will only constitute a subsidy if total costs of air mail service exceed the amount which the users would be willing to pay.

[7] *Review*, pp. 106-07.

been in the black; in 1950 half the lines would have been in the black. Koontz, however, regards 1949 as more normal than 1950,[8] even though most economic time series show a dip for the earlier year. It is this evidence of poor financial performance—that is, what would have been poor financial performance if passenger fares had been paid for the carriage of mail—that leads Koontz to search out its causes (other than his own assumptions) and make his policy recommendations.

There follows a section in which Koontz demonstrates that there are not substantial differences in the average cost per available ton mile of the sixteen carriers.[9] Available ton miles represent total transport capacity, some of which will be utilized, and some left to perish through underutilization. Presumably, however, air line management is producing revenue transportation rather than potential transportation, so that a more relevant calculation might have been average cost per revenue ton mile. But because the average cost per available ton mile does not show great variations between carriers, Koontz concludes that economies of scale are absent, although he points out later that it is variations in the load factor[10] which essentially account for the less favorable earnings in the commercial market of the smaller carriers.[11] This implies that perhaps a smaller proportion of seats are occupied on the smaller lines, which still further implies that possibly the unit is too large. I submit that the matching of the size of the productive unit, in this case the plane, to the size of the market, is one of the problems generally comprehended among those of economies of scale. It is also true, and the industry has shown considerable concern thereon, that no small modern aircraft, suitable to lightly trafficked routes, are available from American manufacturers. Smaller air lines thus are largely forced to employ units larger than can be filled by their traffic. But no amount of corporate merger will solve the technical problem of providing small, fast, and cheap aircraft for thinly trafficked routes.

Koontz then turns to variations in the effectiveness of management as a possible explanation for the variations of profits between carriers. He explains the profitability of Eastern and American as resulting, in part, from the offering of a high volume of frequently scheduled service.[12] If the profits of these two carriers are due to the size of their output, this hardly seems consistent with his statement on the preceding page: "When only the nine largest carriers are considered, evidence of any economies of scale virtually disappears."[13]

Koontz indicates that although there are wide differences in managerial effectiveness, such differences are insufficient to explain the differences in profitability. While that may sometimes be true, there may be other cases in which differences in managerial ability are important. For example, a simple test of managerial ability would seem to be its response to changes in the

[8] *Review*, p. 108.

[9] *Review*, pp. 108-14; *Journal*, pp. 133-40.

[10] *I.e.*, revenue ton miles divided by available ton miles.

[11] *Review*, p. 117; *Journal*, pp. 144-45.

[12] *Review*, pp. 114-15; *Journal*, p. 150.

[13] *Review*, p. 113.

volume of traffic. If output, measured in revenue ton miles, changes, one would expect an alert management to be able to change its costs in the same direction, although, of course, not necessarily proportionately. Thus if traffic declines, management, at least within the limits imposed by the indivisibility of the productive unit, the aircraft, should succeed in making some cost reductions; and likewise, above the minimum size, one would expect management to have to increase its costs somewhat when traffic rises. On heavily travelled routes the qualification of indivisibility may become unimportant.

A leading trade paper, *American Aviation*, publishes monthly data on air line traffic and expenses, taken from reports of the carriers filed currently with the Civil Aeronautics Board. During the period of relative price stability between January 1947 and the outbreak of the Korean War, carriers' cost figures can be used without the clear-cut necessity of a deflator, so that simple correlations of total operating expenses, in unadjusted dollars, and total revenue ton miles can be made for individual carriers. As just indicated, one would expect the well-managed carrier to show a positive relation between changes in revenue traffic and changes in total cost. For the period for which I have scrutinized the data, January 1947 to May 1950, twenty-nine observations or twenty-eight intermonthly periods, tightly managed and profitable Eastern had such a positive relation 20 times, and negative only 8. United had 17 positive relations and 11 negative. TWA, least profitable of the large carriers, had a positive only 13 times, and negative 15 times. Thus it was more often than not the experience for TWA, at least during those 29 months, to spend more when traffic declined and to cut expenditures when traffic rose. I think it can be said that differences of this magnitude in managements' responses to changed conditions may well be sufficient to account for the differences in profitability, at least as between the particular carriers mentioned. That difference in profitability had been substantial.

Koontz then suggests that load factor is the most important factor in financial self-sufficiency, and that high load factors may be obtained by serving major markets.[14] This may be another way of saying the aircraft in conventional use are too large for the market they serve at the existing price level. But let us examine a moment the first proposition: that high load factors mean profitability. Koontz' Table V, showing, for 1949 and 1950 for each trunk carrier, actual load factor and net operating income adjusted by his method, that is, by charging passenger fares to the mail load, is the basis for his assertion that improved load factors are the solution. He points out that most lines could have operated profitably, even after mail pay adjustment, if they had had the load factor of American, United, or TWA, about 58 per cent in 1949 and a few points higher in 1950. But he ignores his figures which indicate that Eastern operated profitably on a 46 per cent load factor in 1949; and 53 per cent in 1950. For 1949, the lines with higher load factors than Eastern's, but nevertheless with losses after mail pay adjustment, represent nine of the twelve other trunk carriers, though in 1950 only two were clearly inconsistent with his position. Thus, while load factors must be an important factor in profit-

[14] *Review*, pp. 117-22; *Journal*, pp. 144-45.

ability, other explanations cannot be discarded. The profitability of Eastern has still another aspect which is awkward for his thesis. He ascribes Eastern's profitability to its "policy of offering a high volume of frequently scheduled service."[15] There is nothing inconsistent with frequent service and low load factors, but can Koontz mean that most lines are profitable because of high load factors, and Eastern is profitable because of low load factors?

Koontz concludes that there is a high degree of correlation between large airlines and service in large markets, defined as the top 97 pairs of stations, in rank order of traffic generation. Such a conclusion would appear tautological, the same statistical unit, volume traffic, accounting for both large carriers and large traffic centers. It is, however, not inconceivable that the profitability of the large carriers serving large markets is due, to the extent that it is related to the size of the markets served at all, to the service of few small markets rather than many large. Koontz offers no direct evidence at this point.

There is, at any rate, still further room for question of his hypothesis: "The importance of having the high traffic density pairs of cities on a carrier's routes cannot be overestimated. Heavy travel brings favorable load factors, particularly where aircraft can be scheduled nonstop between the high traffic generating pairs of cities."[16] The exceptions to this proposition, as he indicates,[17] are TWA, National, and Western, which carriers served the so-called productive markets but did not get their "prorated share" as calculated in Table VI.[18] That failure to get the "prorated share" is ascribed to equipment shortages,[19] or in other words, a shortage on the supply side. If failure to get the "prorated share" of the market were due to such supply shortages, we would expect to find such supply as was available to be fully utilized. However, his Table V[20] shows the following load factors:

Revenue Load Factor Domestic Trunk Air Lines
(per cent)

	1949		1950	
	Total	Passenger	Total	Passenger
Trunk line average	53.4	59.2	57.2	62.7
Trans World	58.4	60.9	61.0	65.4
National	37.5	44.9	47.2	53.6
Western	49.5	50.6	56.7	57.2

Thus, while TWA is above the industry average, National and Western are below. It is hard to believe that it is a shortage on the supply side that causes Western and National, at least, to sell a below average share of their available

[15] *Review*, p. 114.

[16] *Review*, pp. 118-19. See also, *Journal*, p. 145.

[17] *Review*, p. 121. Northeast was also an exception.

[18] *Review*, p. 119; *Journal*, Table 8, p. 146.

[19] *Review*, p. 121; *Journal*, pp. 150-51.

[20] *Review*, p. 116; *Journal*, Table 4, p. 135, shows total load factors for 1949 only.

output, and the reader might almost suspect that Koontz has confused demand and supply.

Since there seem to be certain deficiencies in the reasoning which underlies his diagnosis of the ills of the industry, it may be unnecessary to discuss here the merits of his policy recommendations. We might point out that it is his position that those carriers which are in difficulties are in that position because of the nature of the market they face; that is, the problem is one of demand. Will those problems be alleviated by the proposal to change the structure of the industry, that is, to make changes in the supply pattern?

It is, of course, always possible that a helpful prescription may nevertheless follow from a faulty diagnosis, and the merger program which Koontz advocates should receive, and has received, considerable attention. There is a great need for both careful analysis and fresh imagination with regard to our air transport policy.

John P. Carter*

* The author is associate professor of business administration, University of California.

Air Line Self-Sufficiency: Rejoinder

I welcome this opportunity to reply to Mr. Carter's interesting comments on my articles concerning the problem of financial self-sufficiency of the domestic trunk air lines.[1] It is gratifying to have these papers arouse such interest, although I wish that I might have found in my colleague's analysis more which would, in my judgment, contribute to the solution of the complex public policy questions involved in air line subsidies.

Except for Carter's point with regard to matching the size of the aircraft to the traffic available (an impracticable half-truth as will be pointed out), none of the comments he makes affects the validity of any of the conclusions reached in my article. Nor does he so claim in any of his detailed criticisms. Nevertheless, I should like to take the liberty of dealing briefly with certain of Carter's comments in the interest of clarifying aspects of my analysis which he apparently has misunderstood.

In the first place, Carter questions my use of the passenger return as a basis for measuring the extent of excessive mail pay and comes to the unchallenged conclusion that the actual extent of subsidy is indeterminate. It is exactly because this is true, as Ernst and Ernst found out after a long and expensive study,[2] that I chose not to devote my article to the futile task of exactly determining the amount of subsidy. As was clearly set forth in my paper, this assumption was made as "a working hypothesis" and "without presuming to decide what a service mail rate (*i.e.*, one free of an element of subsidy) should be."[3]

As I further made clear in using this assumption, I was employing what

[1] Although Mr. Carter makes references to another article written by me, his argument appears to be based on the paper in the *Review*.

[2] My assumption was made prior to the completion of the study of Ernst and Ernst.

[3] *Review*, p. 105.

might be regarded as a *maximum* service rate for purposes of clearing away some of the camouflage of excessive mail pay. Carter does not argue that this assumption vitiates the general validity of the net incomes resulting or the conclusions I drew. On the contrary, if there is more of a subsidy element than results from this assumption, my thesis is strengthened.

He questions my reference to 1949 as being a more normal year for the air lines than 1950. I fail to see how anyone familiar with the extraordinary traffic demands generated by the defense activities, which were expanded when the Korean conflict broke out in June 1950, can believe that 1950 would be more normal than 1949. Certainly no one in the air line business has had illusions on this point. However, using 1949 as a more normal year than 1950 for the air lines does not imply that air line traffic had reached its peak volume in that year. The point is rather that the sudden increase in demand for air travel, particularly over certain high traffic density segments, pushed load factors above that level which experience and service requirements have proved to be normal.

Carter prefers to use the revenue ton mile, rather than the available ton mile, as the statistical base against which to measure and compare operating costs of the various carriers, although he does not show that the use of this base would affect my conclusions. There is no unanimity among air line economists on this point, although most economists and statisticians in the industry feel that the available ton mile is the more accurate measure of what an air line produces, as a producing unit.[4] While any air line manager would naturally prefer a high volume of revenue ton miles (depending, of course, on the revenue per ton mile) to a high volume of available ton miles, the latter appears to be a more accurate basis upon which to compare operating costs because it is relatively independent of market factors.[5]

Suppose, for example, that air line A flies a Convair airplane at a direct operating expense of 80¢ per plane mile and air line B flies the same airplane under similar conditions at a cost of 60¢ per plane mile. Assuming a four-ton available load, the direct expense per available ton mile is 20¢ in the case of A and 15¢ in the case of B. But suppose that air line A happens to be operating in a dense traffic market and sells all its space while air line B operates in a more normal market and sells only an average of 60 per cent of its space. In this case, the cost per revenue ton mile would be 20¢ for A and 25¢ for B. From the standpoint of efficiency of operation, rather than from the point of view of fortuity of market, would Carter say that air line A is the more efficient operator?

Carter states that the problem of the smaller air lines operating over low density routes is not load factor but rather the managerial and technical one of matching size of the productive unit (the plane) to the size of the market. If it were not for the inexorable character of facts, Carter's point would be persuasive. No manufacturer of airplanes has yet been able to produce an air-

[4] For example, cost comparisons computed for industry use by the Air Transport Association employ the plane mile and the available ton mile as statistical bases of measurement. Most managements likewise use these bases for this purpose.

[5] In preferring the revenue ton mile basis, could Carter be confusing demand and supply factors?

plane which could conveniently expand and contract in size—and with a proportionate flexibility of operating expenses—to accommodate variations in passenger and cargo load between points on a flight, or times of a day, days of a week, or weeks of a year. One cannot deny that, if an airplane could be built which would carry few passengers as cheaply per passenger mile as those which carry many, much of the economic problem in air line operation would disappear. Ideally, the kind of flexibility envisioned by Carter would be gained by having single passenger airplanes which would operate as efficiently, as fast, and as safely as airplanes built for 40 or 50 passengers. Unfortunately for the air line industry, this is an impossible technical problem, largely because of certain basic facts of airplane design and air line operation.

While no manufacturer has tried to obtain the ultimate in flexibility—the efficient single passenger air line transport—in recent years practically every manufacturer has considered seriously airplanes of 15-20 passenger capacity, and several have spent millions of dollars on such development, only to find that these are economically impracticable for operation at the level of competitive rates existing in this country.[6] Because of certain minimum standards of safety, performance, and comfort, the minimum economic size of a transport airplane is sharply limited. A plane must have at least two engines, must have minimum characteristics of stability, and other minimums with respect to such equipment as radio, hydraulic and pressurization devices, and electrical systems. Thus, to build a 20-passenger airplane would cost approximately three-quarters as much as a 40-passenger airplane.[7] Moreover, there are certain operating costs beyond those related to the cost of the airplane (such as depreciation and maintenance) which tend to place a floor under economic size. Crew salaries and expenses are an important element of operating expenses and the requirements for passenger transport operation are a minimum of pilot, copilot, and cabin attendant. Thus the small airplane Carter relies upon for his matching of the unit with traffic would have practically the same crew costs per plane mile as a 40-passenger airplane.

Carter's point that a simple test of managerial ability would be its response to changes in volume of traffic has a certain degree of validity and is one well worth studying.[8] He shows, as between Eastern, United, and TWA, that Eastern had the best record on this score and TWA the worst. I have come to similar conclusions. But does his measure show the reason why Eastern was profitable and the twelve smallest carriers had far less profitable records? His observation, albeit interesting, has no particular bearing on the problem to which my paper was addressed.

[6] Some of the interesting failures in this class of airplane, all built since World War II, are the Lockheed "Saturn," the Beech 34, the Boeing 417, and the Northrop "Pioneer." Those interested in Carter's point would do well to study the economic and technical problems of these ventures. Even the successful Douglas DC-3 would never be built at today's manufacturing costs, its low plane-mile costs (which still are not low enough to give very favorable seat-mile costs) being due largely to the low capital cost involved.

[7] It is estimated that a 21-passenger DC-3, designed to present standards of safety and performance, would cost approximately $400,000 to build now compared to the selling price of a 40-44-passenger Convair liner of $540,000.

[8] As is noted below, because of competitive problems and service difficulties this however may not be as easy in practice as Carter implies.

Carter distorts some observations made as to variations in the effectiveness of management when he questions the consistency of my references to the "high volume of frequently scheduled service" of Eastern and American and my references to the generally small differences in the costs per available ton mile among the nine largest carriers. These being facts and not judgments, his comment is curious. As I stated in my paper,[9] there is some real question as to whether these cost differences should be as small as they are, except, of course, in the case of Eastern whose fairly low cost record I repeatedly mentioned.

Carter appears unclear about some of the economic characteristics of air line operation.[10] He finds the relationship of high volume of frequently scheduled service to revenue generation puzzling. He questions how the quality and volume of service offered (the "supply side") can affect the size of the market captured and how a carrier can be thwarted in obtaining its share because of equipment shortages. He feels that there can be no shortage of equipment if a carrier's load factor lags markedly behind the industry average and voices the suspicion that I may have confused demand and supply.

If the air line market were perfectly competitive, Carter's suspicion might be justified. But this is not the case, for it is obvious that air line competition is imperfect. Surely he has not forgotten that, under imperfect competition, the demand curve facing the individual firm can be shifted by the promotional action of the firm itself. It is one of the simplest facts of transportation that, where two or more carriers serve a market, the carrier offering superior volume and frequency of service has a marked advantage which is normally greater than the service difference.[11] Naturally an air line has difficulty in offering

[9] Note page 114 where I say, "It is difficult to conceive that most of the economies of size have been utilized when air lines reach the size of the small or medium small air lines listed in Tables I and II. It is certainly open to question that there should be so little difference in costs per available ton mile between air lines the size of Braniff or Western and those the size of American or United, which are seven to fourteen times as large. One gets the impression that . . . cost control is the more effective the more the need for it."

[10] I cannot comprehend his implication that I have argued that "most lines are profitable because of high load factors and Eastern is profitable because of low load factors." As pointed out throughout my study, Eastern is in some respects an exceptional case. In any event, Carter would surely not argue that, had Eastern's load factors been higher, its profits would have been less. Nor does he challenge the *essential* point made that the weaker carriers would have earned a profit, after adjustment for mail pay, had they been able to attract traffic adequate to bring about the load factors enjoyed by American.

[11] This is a familiar phenomenon in transportation and many instances could be cited to illustrate it if space permitted. While it has certain advantages other than frequency, the far greater frequency of service offered by the Pennsylvania Railroad than by the Baltimore and Ohio between New York and Washington has been a major factor in the higher *average* passenger loads of the Pennsylvania between these points. During 1949, National Airlines offered few through flights between New York and Miami, while Eastern offered many; naturally the travelling public utilized Eastern more than proportionately to the difference in schedule frequency. While there are service features other than frequency which attract passengers, frequency, as every air line sales manager knows, is one of the most important sales weapons. And many a railroad or air line passenger operator who sought to curtail expenses by reducing frequency has found to his sorrow that he suffered a greater than proportionate loss in traffic.

adequate service in a given market if it does not have the equipment to fly the necessary schedules.[12]

In the air line business, as in other similarly regulated industries, a management may not freely choose its market. Unlike businesses whose entry is relatively unrestricted by government, an air line cannot decide that, since the market facing it is thin, it will exploit a richer and more dense traffic area. It is free, of course, to apply to the Civil Aeronautics Board for admission to these markets and the Board's docket has been crowded throughout its history with applications of this kind. Even though the Board has granted a small percentage of these many applications for new routes and route modifications, its administration of a government policy of controlled competition has sharply limited the ability of the weaker carriers to enter many of the rich traffic markets of the larger and stronger carriers. If it were not for the restraining hand of government, these weaker carriers would take a page from the book of the noncertificated coach lines and attempt to enter the more profitable traffic markets.

If the weaker air lines are doomed to a subsidy existence because they cannot reach the heavy traffic-generating segments of the American economy, and thereby receive enough profitable traffic to offset losses on the thin traffic segments, the solution is obviously to open better markets to them. This can be done through abandonment of the present regulatory policy of controlling entry, or through remaking the route pattern and reallocating fewer routes on a more balanced basis, or through a wise program of combination. The last has seemed to me to be the most prudent course for public policy.

HAROLD D. KOONTZ*

[12] The concept and significance of load factor appears to have eluded Carter. The productive unit not being a perfectly divisible one, changes to meet shifts in demand between stations and between times of day and year cannot be made to the degree necessary to sell all available ton miles or seat miles, unless, of course, a "backlog" of passengers and cargo continually exist (a situation which is approached on some high density traffic segments.) Management is often sharply limited in its ability to curtail service in order to improve loads. A low load factor is often an indication of inadequate service, rather than of too much service.

* The author is professor of business policy and transportation in the School of Business Administration, University of California, Los Angeles.

Underemployment Equilibrium Rates of Growth: Comment

In his recent article,[1] Professor Eisner has endeavored to extend the work of Professor Domar by examining the nature of underemployment growth equilibria, as opposed to Domar's full employment equilibrium.[2] Despite his rather confident assertion that "would-be objectors to the theoretical concept

[1] Robert Eisner, "Underemployment Equilibrium Rates of Growth," *Am. Econ. Rev.* (March 1952), XLII, 43-58.

[2] *Cf.* E. D. Domar, "Capital Expansion, Rate of Growth and Employment," *Econometrica* (April 1946) XIV, 137-47; *idem*, "Expansion and Employment," *Am. Econ Rev.* (March 1947), XXXVII, 34-55; and *idem*, "The Problem of Capital Accumulation," *ibid.*, (Dec. 1948), XXXVIII, 777-94.

of underemployment equilibrium rates of growth . . . may . . . find little tangible to attack,"[3] a good deal, if not most, of Eisner's analysis seems subject to dispute. The points at issue are a series of misleading and questionable assumptions, some formal, others substantive, which cast considerable doubt on the validity of his conclusions. For convenience, we make the following comments under subheadings which refer to the appropriate equations and figures in Professor Eisner's article.

1. *Equation (17) and Figure (1):*

$$\frac{dP}{P} = \alpha\sigma\frac{Y}{P} \tag{17}$$

where P stands for productive capacity, α is a constant average (and therefore marginal) propensity to save, and σ represents "the ratio of the change in productive capacity . . . to the change in capital stock,"[4] which henceforth we shall refer to as the "productivity ratio."

Our first comment, which bears upon Domar's analysis also, concerns the reference to $\alpha\sigma$ as a full employment growth rate. This would be true only in the event that the growth in the labor supply is the same as the growth in capital stock. If the former is greater than the latter, it is clear that $\alpha\sigma$ will simply be a full *capacity* rate of growth only.[5] If the latter is greater than the former, as suggested by Domar,[6] it is also inconceivable that a growth of real income equal to $\alpha\sigma$ would provide full employment. For full capacity growth is clearly a necessary, if insufficient, condition of full employment growth. Nor can this problem be defined away (as Domar has tried to do) by having σ take account of additions to idle capital. Idle capital, unplanned, cannot accumulate indefinitely without severe repercussions on the rate of investment and employment. This point arises again in connection with Eisner's θ, so we shall defer further discussion of it for the moment.

More important, from the point of view of the present discussion, the Eisner analysis of underemployment growth equilibria, as shown in equation (17) and in each of his later, more complicated, cases, treats as constants variables which are actually functionally related to (even) long-run levels of income and employment. Specifically, the savings and investment coefficients, α and σ, are originally defined with reference to full employment. Eisner then proceeds to establish underemployment growth equilibria, as exemplified by equation (17), in which it is implicitly assumed that the full employment values of α and σ will obtain in the presence of underemployment. However, it is clear

[3] Eisner, *op. cit.*, p. 48.

[4] *Ibid.*, p. 44. Symbolically, $\sigma = \Delta Y/\Delta K$, ΔY being the change in (potential) real income or output and ΔK, the change in capital stock or (algebraic) net investment. Written in this fashion, σ is seen to be the reciprocal of the *accelerator*, $\Delta K/\Delta Y$, if the latter is represented *sans* lags. We shall have occasion to make use of these relationships in the discussion ahead.

[5] This distinction between full capacity and full employment growth and some implications have been discussed in my article, "Full Capacity vs. Full Employment Growth," *Quart. Jour. Econ.* (Aug. 1952), LXVI, 444-49.

[6] Domar, "Expansion and Employment," pp. 44-45.

that α, for instance, will tend to fall below its full employment values during periods of chronic underemployment. With the emergence of steady unemployment and excess capacity, with depressions drawn out and prosperities abortive (this is the significance for cyclical behavior of long-run underemployment), profits on the average will tend to be low; certainly they will tend to be much lower as a percentage of income during such periods than they would be in conditions of full employment. Dividends as well as speculative incomes will probably undergo severe contraction. Entrepreneurial and *rentier* incomes in general may be expected to decline as a fraction of income. These tendencies all point to a redistribution of income in favor of high (marginal) spending groups.

At the same time, σ, the productivity ratio, may tend to be higher during periods of underemployment growth than it would be during full capacity growth. In the presence of widespread unemployment, additional labor can be readily obtained to "work" the incremental capital. The ease with which other resources may be mobilized during chronic underemployment implies that the potential output associated with (net) investment will tend to be larger than it would be when the economy is confronted with full employment bottlenecks.

It is evident that an analysis of the forces which determine whether the economy will progress at full employment or suffer some sort of stagnation must take cognizance of the correlations between α and σ and the level of income and employment. The failure of Eisner to do this leaves his analysis of underemployment equilibrium seriously incomplete.[7]

But let us suppose that the values of α and σ under conditions of chronic underemployment could be established. Even then the validity of equation (17) is questionable. Eisner suggests that in underemployment equilibrium "the accumulation of capital . . ., because of its relation to the rate of growth, involves the creation of a constant proportion of excess capacity. . . ."[8] This statement is implicit in (17). It suggests that the increment in capital corresponding to a given increase in income must allow not only for the latter, but also for the maintenance of the constant proportion of excess capacity. But this leads to the implausible conclusion that the absolute amount of excess capacity will be permitted to get larger as national income rises, *despite the absence of any foreseeable use of the excess capacity.*

This last phrase is italicized because a certain amount of excess capacity is often maintained as a means of meeting peak demands in industries confronted with fluctuating demands. Also, in many of the basic industries and public utilities, where economies of scale are common, a certain amount of

[7] Contrary to Eisner's view (p. 50), the criticisms made of his diagrammatic equilibrium analysis need not necessarily apply to the Keynesian " 'saving-investment' curves of the current textbooks. . . ." For these curves may be appropriately drawn to show, for example, a declining marginal propensity to save and marginal propensity to invest as actual income falls below full employment levels and *vice versa*. And, of course, Keynesian analysis, being short run in character, ignores the σ or capacity-creating effects of investment.

[8] Eisner. *op. cit.*, p. 49.

overcapacity may be maintained in anticipation of future growth in demand.[9] Such overcapacity as this is, by definition, planned.[10] As such it differs fundamentally from the kind of overcapacity considered by Eisner. The latter kind, being the product of insufficient growth, of demand deficiency, is inherently unplanned.[11] Hence there is no reason why it should be maintained. Actually, it should have the effect of reducing the rate of (acceleration-induced) investment, thereby accentuating the underemployment tendencies.[12]

Under the circumstances, it is even difficult to envisage a steady, if underemployment, growth rate. A condition of underemployment may be taken as a situation in which a great many people are dissatisfied. This certainly is the significance of such things as excessive inventories, idle plant and equipment, and involuntary unemployment of labor. Dissatisfied people may be expected to take action, action which may at first speed up the movement away from full employment, but which eventually will lead to reversal of the movement. We are thus back in the field of business cycle analysis. Steady growth rates involving underemployment, as are typified by equation (17), appear wholly inconsistent with the essential conditions of underemployment.

2. *Equations (19), (19a), (21), and Figure 2.* Professor Eisner seems to have sensed something of the problems raised by the use of "excess-capacity" term, θ. He has inserted a term into his investment equation (19a) to allow for the depressive effects of excess capacity. However, the form of equations (19) and (19a), and therefore implicitly (21), is open to question, both on the ground of economic content and on that of consistency. Reproducing these equations, we have

$$S = \alpha Y - \beta P \tag{19}$$

$$I = \alpha Y - \beta P \tag{19a}$$

$$\frac{dP}{P}\left(\equiv\frac{I\sigma}{P}\right) = \left(\alpha\frac{Y}{P} - \beta\right)\sigma \tag{21}$$

We may first concentrate on (19a), the investment equation. It is not the growth in productive capacity (P) per se which depresses investment, but

[9] *Cf.* H. B. Chenery, "Overcapacity and the Acceleration Principle," *Econometrica* (Jan. 1952), XX, 1-11.

[10] In fact, if Y/P is used to denote this kind of excess capacity, equation (17) may be utilized to show full capacity growth which gives explicit recognition to it.

[11] To the extent that unplanned excess capacity can be used to meet planned requirements, its deflationary impact might be mitigated. Postulating this development as a generalization, however, would be tantamount to reducing the entire underemployment equilibrium analysis to a tautology.

[12] If it be argued that the kind of "excess capacity" conceived by Eisner in his Θ is the planned kind discussed in the text, then we would reply that violence is being done to the usual meaning accorded the term "excess capacity." In ordinary usage, the latter refers to capacity which is not desired, for which there is no use, which results only from the absence of better foresight, and most important, which is inconsistent with equilibrium. Additions to capacity made for reasons mentioned above are not excess capacity, and they are useless in explaining chronic underemployment. As one reader has well put it, the use of Θ to represent these planned increments in capital amounts to "slipping in Θ as a verbal disguise for a reduction in σ, but it does not represent any sort of illumination of the underemployment problem."

rather the growth of capital or capacity relative to aggregate output, *i.e.*, excess capacity, which does so. Use of the term P in (19a) probably stems from writings which claim a negative partial correlation between capital stock (or productive capacity) and investment. This view, however, overlooks the reason for this relationship, namely, the lag in the growth of income behind capital (the basic problem of the Harrod-Domar analyses), as a result of which excess capacity develops.[13]

Moreover, (19a) gives the impression that it represents an (admittedly simplified) investment function. Clearly, it is not. The attempt to make equations (19) and (19a) identical stems from the earlier Domar analysis in which the long-run saving-investment function αY is utilized. This function derives its plausibility from the Kuznets secular data showing (net) capital formation to have been a constant proportion of national income over long periods.[14] But the designation of capital formation as some fraction of income, αY, certainly does not mean that αY represents the "true" saving or, particularly, investment functions.[15] Any suggestion as a general proposition that at equilibrium the equality of planned saving and investment and/or planned and realized investment implies that these variables are functions of the same determinants is patently incorrect. Nevertheless, this is what is misleadingly implied by the elaborated short-run saving and investment equations represented by (19) and (19a). These equations notwithstanding, it is a simple matter to show that the equality of the quantities just mentioned implied by steady growth is attainable with saving and investment functions having nothing to do with each other.[16]

If it be argued that the equality of saving and investment assumed at the

[13] Nowhere is this oversight more important than in those discussions in which the marginal efficiency of capital is described as falling as the volume of investment increases. Such discussions, of course, take the level of income as a parameter. Should income increase in proportion to investment, however, it is clear that the resulting shifts in the entire marginal efficiency function would keep the rate of return unchanged as the volume of investment increases. *Cf.* my article, "The Accelerator in Income Analysis," *Quart. Jour. Econ.* (Nov. 1952), LXVI, 592-96.

[14] *Cf.* S. Kuznets, *National Product since 1869* (New York, 1946), p. 118. Actually, the fraction of real income thus saved displays a secular tendency to fall, a point ignored by many writers on saving and income.

[15] As will be shown below (p. 383), αY may be used to denote a long-run saving function.

[16] A simple first order difference equation may be employed in demonstration of this fact. Assume the income model

$$Y_t = (1-\alpha)Y_t + \lambda(Y_t - Y_{t-1}) \tag{a}$$

where λ is the annual accelerator. This equation has as its solution

$$Y_t = \left(\frac{\lambda}{\lambda-\alpha}\right)^t Y_0 \tag{b}$$

where Y_0 is the initial income and must be given. If we assume that $\lambda = 2.5$ and $\alpha = .1$, $\lambda/\lambda\text{-}\alpha$ has a value of .04. This means that if income grows initially in equation (a) at the compound rate of 4 per cent a year, this steady growth will be maintained. At the same time, saving and investment will be equal, even though the former is a function of the income *level* and the latter of the *change* in income.

outset of the discussion makes it permissible to identify the investment function with the saving function, then it may be asserted with equal force that the two functions should take the form of the investment function. Thus equation (19a) may be rewritten in some form such as

$$I = \lambda \Delta Y - \beta \frac{P}{Y} \tag{19b}$$

The first term on the right hand side of (19b) represents acceleration-induced investment, λ denoting the accelerator. P/Y is the ratio of productive capacity to income (output); it replaces the misleading "P" term of (19a). Equation (19b) has the advantage of according completely with the Harrod-Domar models, which are based on the accelerator.[17] In the event that (19b) were adopted, Eisner's equations (16) and (21) would have to be modified thus (remembering that λ is the reciprocal of σ):

$$dP \equiv \frac{I}{\lambda} = \Delta Y - \frac{\beta}{\lambda} P/Y \tag{16a}$$

$$\frac{dP}{P} \equiv \frac{I\sigma}{P} = \frac{\Delta Y - \beta\sigma P/Y}{P}$$

$$= \frac{\Delta Y}{P} - \frac{\beta\sigma}{Y} \tag{21a}$$

While (19b) may not be the only alternative to (19a), it does seem more realistic as an investment equation.

[17] That this is so may be shown as follows:

Write $$I = \lambda \Delta Y \tag{c}$$

then $$\Delta Y = \frac{I}{\lambda} \tag{d}$$

From the formula for the multiplier, $\Delta Y = \Delta I/\alpha$, it follows that

$$\frac{\Delta I}{\alpha} = \frac{I}{\lambda} \tag{e}$$

or $$\frac{\Delta I}{I} = \frac{\alpha}{\lambda} \tag{f}$$

So long as the average and marginal propensities to save are equal, it may be shown that $\Delta I/I = \Delta Y/Y$; cf. Eisner, *op. cit.*, p. 45. $\Delta Y/Y = \alpha/\lambda$ is the Harrod version of the growth formula. But if λ is the reciprocal of σ, then we may write $\Delta Y/Y = \alpha\sigma$, the Domar version of the formula for full capacity growth. Parenthetically, it might be noted that in the event of a disparity between the average and marginal propensities to save, the growth of investment necessary to achieve the "required" or "warranted" growth of income will be a *multiple* of the latter. Thus if s is the average propensity to save and α the marginal propensity, we may write $\Delta I/I = \alpha/s\ (\Delta Y/Y)$. Since, if anything, $\alpha > s$, $\alpha/s > 1$. This proposition may be proved by making the proper adjustments in Eisner's equation (5), *loc. cit.*

The reader's attention is also drawn to the use made of the reciprocal of Eisner's "employment ratio" Y/P. In this way, the second term in (19b) will get larger and investment smaller as the ratio of productive capacity to income (*i.e., excess* capacity) increases. This is in accord with Eisner's objectives.

Turning now to equation (19), the saving equation, two brief comments upon it are pertinent. In the first place, making saving (or consumption) a function of productive capacity as such seems untenable; there is no way in which saving can be related to *potential* output, which is what P symbolizes.[18] Nor would a term such as βP provide an answer to the "difficulties encountered in attempts to derive a long-run saving function from time series data."[19] There would still remain the problem of explaining why saving rises in a constant proportion to P. Such explanations as the Duesenberry "Veblen effects" and/or the Kuznets-Smithies effects of the population growth, urbanization, and new products are still needed.

Secondly, there seems to be a redundancy in Eisner's reasoning in connection with equation (19). This equation ostensibly drops the assumption that saving is a constant proportion of income, αY. Further, the constant in the typical short-run saving function is replaced by the trend term βP. This trend term gives formal expression to "the hypothesis that there is no long-run change in the proportion of income saved."[20] If this is true, it would appear that we are back where we started. This is the same as saying that in the long run, saving rises along a linear saving function which passes through the origin, implying that the ratio of its coordinates and its slope, *i.e.*, average and marginal propensities to save, are the same. This is precisely what the saving coefficient, α, initially implied, for α, in the term αY, is a long-run constant average (and therefore marginal) propensity to save. αY is merely a short-cut representation of a more elaborate short-run saving function which contains a trend term describing the familiar secular downward drift of this function.[21] The upshot of this is that, fundamentally, equations (19) through (21), as Eisner has presented them, do not appear to constitute a net advance beyond equations (16)-(17) if α is correctly interpreted.[22]

[18] However, it might be possible to relate saving indirectly to a (negative) term such as P/Y. Thus if a value for this ratio much greater than unity were to be used to denote chronic unemployment, the depressive effects on the savings coefficient of chronic unemployment discussed earlier might be interpolated into a saving function containing the term P/Y. Eisner has done just this in his equation (22), though using the reciprocal of P/Y and hence making it a positive term.

[19] Eisner, *op. cit.* p. 53.

[20] *Loc. cit.*, footnote 9.

[21] A graphical illustration of this secular drift of the short-run saving function along a long-run function through the origin is to be found in J. S. Duesenberry, *Income, Saving anl the Theory of Consumer Behavior* (Cambridge, Mass., 1949), p. 114. See also P. A. Samuelson, "Full Employment after the War," in S. E. Harris, ed., *Postwar Economic Problems* (New York 1943), p. 35. Though both actually deal with the consumption function, the appropriate adjustments are easily made.

[22] The only way of obtaining a saving function containing a constant marginal, but variable average, propensity to save is with a linear function which intercepts the S axis below the origin; such a function is indicated by Eisner's equation (18). Proceeding within Eisner's own framework, it will be seen that if (18) is also used to denote the investment equation and is subjected to the same kind of differentiation process as that indicated in Eisner's equations (16) through (17) (p. 51), the results will be identical with those of Eisner's first model (p. 51), because of the disappearance of the constants in the course of differentiation. Thus a model based on the stated objectives of Eisner's second model (equations (18)-(21)) yields no different results from those of his initial model.

Most of the comments made in connection with equations (19)-(21) apply equally to Eisner's equations (22) and (22a). Hence we shall not undertake a detailed analysis of the latter. However, his insertion of a term (Y/P) into (22) to show the effects of stagnation on the long-run saving function is a point of interest to income analysts and stagnation theorists alike.

D. HAMBERG*

In connection with this point, the reader's attention is drawn to the difference in reasoning between Mr. Wagner and myself, despite the fact that we have both concluded that the βP term in Eisner's equation (19) vitiates his assertion that he has dropped the assumption of equality between the average and marginal propensity to consume. As a matter of fact, I find myself in agreement with Eisner's reply to Wagner, *viz.*, that equation (7) specifies an equilibrium condition, while (19) specifies a saving schedule or function; hence though P be a function of Y in (7), this certainly is not true in (19). Nevertheless, on the grounds I have argued, (19) does implicitly make the average and marginal values of the saving function equal. (Note the similarity between Eisner's reply to Wagner and my objection to Eisner's making savings and investment functions of the same things as a result of the equality of savings and investment in equilibrium.)

* The author is assistant professor of economics at the University of Maryland.

Underemployment Equilibrium Rates of Growth: Further Comment

Mr. Eisner, in his article in the March 1952 issue of the *Review*,[1] discusses three cases of the determination of equilibrium rates of growth. The first case assumes that the average propensity to save (APS) equals the marginal propensity to save (MPS). An equilibrium rate of growth implies an equilibrium "ratio of income to productive capacity (or 'employment ratio')" (p. 52). However, using this conception of equilibrium, Eisner is not entirely correct in stating, when he takes up his second and third cases: ". . . we drop the assumption that investment is a constant proportion of income but we retain a constant marginal propensity to save." As we shall see, APS = MPS in all three cases when either equilibrium[2] or any given employment ratio Y/P exists.

His equation (18) is the traditional savings function:

$$S = \alpha Y - b.$$

Since b is a constant, obviously APS $\neq$ MPS. But Eisner modifies (18) by substituting βP for b.

However, in equilibrium[3] P is *not* a constant; rather it is a function of Y,[4] as stated in equation (7) in the article:

[1] Robert Eisner, "Underemployment Equilibrium Rates of Growth," *Am. Econ. Rev.* (March 1952), XLII, 43-58.

[2] Refer to equations (7), (7a), and (8), *ibid.*, p. 46.

[3] If one considers the employment ratio as given $\widehat{\Theta} = Y/P$, the subsequent analysis follows analogously when $\widehat{\Theta}$ is substituted for Θ.

[4] Note that Eisner is perfectly correct in stating that APS is not a constant proportion of income if both Y and P are considered independent variables of the savings functions (equations 19 and 22). But in a position of equilibrium as defined in the article, Y and P are not functionally independent; in Eisner's words, ". . . if we start from a position of equilibrium such that $Y_o = \Theta P_o$, maintenance of equilibrium requires that $\Delta Y = \Theta \Delta P$" (p. 46).

$Y = \theta P \qquad [O < \theta \leq 1$, the equality holding in full employment].

Hence equations (19) and (22) should be rewritten:

$$S = Y(\alpha - \beta/\theta) \qquad (19^*)$$
$$S = Y(\alpha\theta - \beta/\theta) \qquad (22^*)$$

From these equations one can simply prove that APS = MPS:[5]

$$MPS = \frac{dS}{dY} = \alpha - \beta/\theta \qquad (19^{**})$$

$$APS = \frac{S}{Y} = \alpha - \beta/\theta \qquad (19^{***})$$

$$MPS = \alpha\theta - \beta/\theta \qquad (22^{**})$$

$$APS = \alpha\theta - \beta/\theta \qquad (22^{***})$$

Since MPS is assumed to be a constant, then α, β, and θ are constants; hence we conclude that APS is also a constant.

As a consequence of the equality of the marginal and average propensities to save, Mr. Eisner's algebraic manipulations for deriving equations (21) and (23) can be simplified. One need only substitute the above values of MPS (or the equivalent APS) for α in equation (17) of the first case. The substitution is justified since the first case assumes APS = MPS, and this note has shown that the same equality exists in the second and third cases when either equilibrium prevails or an employment ratio is specified.

HARVEY M. WAGNER*

[5] As a result of the equality, Mr. Eisner's use of α may be confusing. In his first case, α = MPS = APS. If Y and P varied independently, then α = MPS in the second and third cases also. But in an equilibrium context, α is merely a parametric constant $\neq$MPS = APS. (See footnote 4.)

* The author is a student at Stanford University.

Rejoinder

I welcome this opportunity to attempt to clear up what seem to me to be certain grievous misunderstandings of the arguments set forth in my "Underemployment . . ." article. These misunderstandings relate, I fear, not merely to my own work but to our general theories of growth of income and determination of the level of income at a given time.

Reply to Wagner. Wagner's departure from my analysis stems from his statement that "in equilibrium, *P* is *not* a constant; rather it is a function of *Y*." My equation (7), that $Y = \theta P$, is merely a formal description of "equilibrium" as I have defined it. It is most assuredly *not* a statement of how *P* may be expected to vary in relation to *Y* (or vice versa). The crux of the whole problem of economic growth as discussed by Domar and Harrod, as I

hoped I had suggested in my own brief comparison of their models, is that there is no reason to be confident that this "equilibrium," either of Domar's full employment variety or of the underemployment varieties constructed by Harrod and myself, would be maintained. Harrod, indeed, goes to great pains to indicate that growth equilibria are essentially unstable, and in so doing, lays the groundwork for the Hicksian model of the trade cycle. An essential purpose of my article was to clarify the implications of a hypothetical maintenance of a constant rate of growth. I was most certainly not indicating that our economic system is characterized by any such constant rate.

Hence it is quite at variance with my analysis to assume that such a constant rate exists and then to rewrite our traditional economic functions on the basis of that assumption. Specifically, we are accustomed to stating that saving is a function of income. Occasionally we choose to add certain other variables, as I have done, and say that saving varies in some relation to changes in these other variables. However, it is certainly foreign to the notion of a saving function to relate the variation of saving to changes in income under the rigid assumption that income remains a constant proportion of productive capacity. This, of course, *can* be done, and I shall point in a moment to some value that may come from such a relation. But, I believe, to refer to such a relation as a saving function, or to refer to the derivative of saving with respect to income, obtained from such a function, as the "marginal propensity to save," as has Wagner, is to do violence to our tools of analysis.

The point to which Wagner might better have called attention is as follows. If σ is constant, that is, if the increase in productive capacity accompanying units of investment remains constant, maintenance of a constant employment ratio (income divided by productive capacity) at a fixed rate of growth requires that saving (investment) remain a fixed proportion of income. However, this assuredly does not mean that the marginal propensity to save *is* constant. For, expressing saving as a unique function of income, we would have to say that the curve representing this function on the two dimensional income-saving chart moves as productive capacity changes. The slope of this (moving) income-saving curve at any moment of time is the marginal propensity to save and it *is* constant in my equations (2b), (18), and (19). In equation (22) the marginal propensity to save ceases to be constant; partial derivation reveals quickly that $\frac{\partial S}{\partial Y} = \frac{2\alpha Y}{P}$. In none of the cases other than the most simple one, in which, by assumption, $S = \alpha Y$, does the marginal propensity to save equal the ratio of saving to income (Wagner's "average propensity to save"). Far from confirming even the constancy of the proportion of income saved, my departure from the simplified function (2b), $S = \alpha Y$, should suggest that this constancy is dependent upon the satisfaction of the conditions of growth which I have defined as "equilibrium." To allow the saving function, or the marginal propensity to save, to be defined by the condition of equilibrium which I have assumed, would be no more appropriate than to define the saving function in the Keynesian model, to which I have

tried to make my system analogous, as the (moving) intersection of (moving) saving and investment curves.[1]

Reply to Hamberg. The essential part of my answer to Hamberg must be to call attention again to my article and to the articles of Domar from which my own analysis is an offshoot. For, though objecting to my "misleading and questionable assumptions" (as well as those of Domar), Hamberg shows evidence of failing to understand the nature of those assumptions. By misinterpreting definitions and symbols he devotes much of his comment, curiously, to charging me with ignoring just those aspects of the problem to which much of my article was explicitly directed:

1. In his "first comment" Hamberg claims that $\alpha\sigma$ is "a full employment growth rate . . . only in the event that the growth in the labor supply is the same as the growth in capital stock." He reveals here an unfortunate confusion as to the meaning of σ. For as Domar has defined it, and as I reminded readers in my article, σ is the ratio of the change in productive capacity in a period of time to the change in capital stock which takes place in that period of time; it is the change in capacity which *accompanies,* not the change in capacity which *is caused by* a change in capital. Symbolically, σ is $\frac{\Delta P}{\Delta K}$, not $\frac{\partial P}{\partial K}$.

What Hamberg apparently has in mind is $\frac{\partial P}{\partial K}$, the familiar marginal productivity of capital.[2] As I pointed out in my article, "The wording of this definition [of σ], it should be noted, is such that σ may well be a function of other variables than investment. In particular, increases in population which involved increases in the supply of labor would raise the value of σ."[3]

Thus, if productivity per worker were growing at a 2 per cent rate as a result of capital investment, change in technique, and all other factors, and if the labor supply were growing at a 1 per cent rate, the aggregate growth in capacity (or the full employment rate of growth) would be 3 per cent (more exactly, of course, 3.02 per cent). And this, in Domar's terms, would be precisely $\alpha\sigma$. This might have been viewed as a combination of the factors, $\alpha = .2$ and $\sigma = .15$. Now, if the rate of growth of the labor supply were to increase to, say, 3 per cent, this would in no way destroy the correspondence between $\alpha\sigma$ and the capacity rate of growth of income. Rather, the value of σ would be increased. For the increase in capacity accompanying each unit of investment would now reflect, in addition to the direct results of the

[1] On a minor point which I hope the foregoing makes irrelevant, I fail to see why Wagner claims that his note enables us to derive my equations (21) and (23) more directly. These equations were each "derived" in one step, by simple substitution. Wagner's development would seem, in addition to being spurious, to have actually certain undesirable qualities of roundaboutness.

[2] Curiously, this is exactly the confusion warned against by Domar. See, for example, his "Expansion and Employment," *Am. Econ. Rev.*, March, 1947, XXXVII, 40, where he cautions: "σ should not be confused with other related concepts such as the traditional marginal productivity of capital."

[3] P. 44, fn. 2.

investment itself (the marginal productivity of capital) and the other factors increasing productivity per worker, the 3 per cent increase in the labor supply. The capacity rate of growth of income would thus be 5 per cent (more exactly, 5.06 per cent), and assuming that the value of α remained .2, the value of σ would have been raised from .15 to .25.

It should be understood that the basic point of the Domar model is simply that growth in *capacity*, but not necessarily growth in *output*, accompanies investment. Growth in capacity will be more if other factors, such as an increasing labor supply, operate conjointly with investment to increase capacity. In order for full employment to be maintained, actual output must grow as fast as capacity, a "capacity" which presumes full utilization of all resources including labor. My own departure consists of exploring the consequences for the level of employment of rates of growth other than the rate necessary to maintain full utilization of capacity.

The possibility of underemployment of labor when capital is fully utilized is very likely a real one, but it is a problem of an essentially different nature from those tackled with the Domar model. The Domar problem is to raise demand at a rate equal to the increase in capacity. Hamberg says, in effect, that even if demand does increase at this rate something (structural maladjustments in the economy?) will prevent complete utilization of the labor supply. Presumably, increases in aggregate demand would result in price inflation of investment goods, and for some reason no change in the relative prices of investment goods and of labor would take place to bring about absorption of the increased labor supply. It is possible to explain this within the framework of a model of a stationary economy. It certainly might be true that in an economy with a continuously increasing labor supply, frictions and lags might prevent complete absorption of any given increment of labor before the next increment were thrown into the market. This, however, is all quite outside the basic model used by Domar and by me. Hamberg is not correct in finding any inconsistency on that account in our tools of analysis.[4]

2. Let us consider next Hamberg's criticisms of me for assuming "implicitly . . . that the full employment values of α and σ will obtain in the presence of underemployment." First, I do not see how Hamberg can assert that "it is clear that α, for instance, will tend to fall below its full employment values during periods of chronic underemployment." I wonder if he has not confused α, the marginal propensity to save out of income, with the proportion

[4] I cannot see why Hamberg relates underemployment to an excess of the rate of growth of the labor supply in relation to the rate of growth of capital. There seems no reason to assume that this kind of structural unemployment might not occur if the labor supply were growing less rapidly than capital, or even if the labor supply were not growing at all. (For example, locational, technological, or productwise changes might bring on underemployment which could not be ascribed to a deficiency of aggregate demand.)

It should be stated, in fairness to Domar, that he does consider the possibility that it will be physically impossible to attain the full capacity rate of growth (defined to involve full employment) in "The Problem of Capital Accumulation" (especially pp. 782-84), which Hamberg cites but to which he does not specifically refer. Domar's concept here may well be related to the ceiling to the rate of growth which plays such an important part in the Hicksian trade cycle.

of income saved. His reference to redistribution in periods of low income suggests that a lower proportion of income will be saved as unemployment increases—exactly what my functions (19) and (22) were used to demonstrate.[5] If, in fact, Hamberg has in mind the proportion of income saved, I must reply that I certainly have not assumed, implicitly or otherwise, that the full employment value of this proportion would "obtain in the presence of underemployment." The main burden of the latter part of my paper was to show how the proportion of income saved may vary with the proportion of underemployment and what some of the effects of this variation might be.

I have no objection in principle to Hamberg's hypothesis that σ may not be a constant.[6] My own presentation was intended to offer simple first approximations to the relationships under consideration. It would certainly be in order to examine the implications for the system of improving on these approximations on the basis of further theoretical or empirical considerations. It may be well, however, to evaluate carefully Hamberg's particular observation that σ may tend to be higher in periods of underemployment than during full capacity growth, in the light of Domar's discussion of the difference between σ and s.[7] Hamberg's observation may perhaps be more appropriately restricted to s, the increase in capacity relating to units of investment taken by themselves, abstracting from the possible loss in capacity resulting from displacement of existing capital stock which the new units of capital make uneconomical to operate.

3. In objecting to a concept of growth which "involves the creation of a constant proportion of excess capacity," Hamberg would appear to be misconstruing a tool of analysis as a statement about reality. I nowhere indicate that I believe that any economy can be expected to grow with a constant proportion of excess capacity. Indeed, my basic approach, in common with that of Harrod, Hicks and Domar, is that the equilibria presented would in fact be most difficult if not impossible to maintain. As emphasized in my article, Harrod's "warranted rate of growth," for example, to which my

[5] Indeed, I used considerable space in the original article, much more than it might be appropriate to employ for reiteration, to describe "low levels of investment during periods of unemployment" (p. 54 and p. 54 n), "dis-saving necessary to care for the unemployed" (p. 55), and "investment as a function of income, productive capacity and the employment ratio" (Figure 3, p. 56).

Hamberg's reference "to a redistribution of income in favor of high (marginal) spending groups" in a period of underemployment strikes me as uncertainly grounded empirically but even more doubtful in terms of theoretical relevance to his assertion that the *marginal* propensity to save declines during periods of unemployment. It would seem quite probable that "high spending groups" maintain their high spending in the face of such adversity by borrowing and using past savings. This would hardly involve a low *marginal* propensity to save. But more important, individual or family propensities to save out of disposable income cannot be directly aggregated to obtain functions for the economy as a whole. The increased proportion of income absorbed in unemployment compensation and relief and the redistribution of income itself would both bring a sharp drop in saving as income and employment drop—hence a high aggregate *marginal* propensity to save.

[6] *Cf.* p. 44, fn. 3.

[7] *American Economic Review* (March, 1947), XXXVII, 39-40 and *Econometrica* (April, 1946), XIV, 143-45.

"equilibrium rate" is kin, is explicitly in *un*stable equilibrium; the slightest departure in either direction would set in motion forces accelerating the movement from equilibrium.[8]

Hamberg observes that "a condition of underemployment may be taken as a situation in which a great many people are dissatisfied," the result of which may be to "speed up the movement away from full employment." While not directly relevant to my model, this observation does deserve some comment in so far as it is, in effect, an unwarranted rejection of Harrod's "warranted rate of growth." I can see no valid reason for objection to the notion that a certain amount of aggregate underemployment is consistent with a situation in which some entrepreneurs are disappointed, others are satisfied, and still others achieve results which exceed their expectations. If such a situation can exist, there seems no a priori reason to reject Harrod's hypothesis that there is *some* rate of growth which (at a given time[9]) would induce entrepreneurs to behave in such a way that this rate of growth would be continued.[10] Hamberg might reject this hypothesis by asserting that there are relevant discontinuities in the function relating the aggregate *ex post* rate of growth and resultant behavior, or by asserting that the only rate of growth which would result in such behavior must involve full (or more than full?) employment. In either case, such rejection, without substantial supporting evidence and consideration of the conclusions of analysis by recent writers in the area of growth as well as the more traditional Keynesian underemployment presentation, would appear rather dogmatic.

4. Much of Hamberg's criticism of what he refers to as my "investment equation" is based on a terminological confusion. Like Keynes in the *General Theory* and Harrod and Domar, and like the United States Department of Commerce, I define *ex post* investment as identically equal to *ex post* saving; this equality is not dependent upon any equilibrium condition. My saving functions relate the amount of saving that would be *realized* at different levels of income and/or the other relevant parameters. The marginal propensity to save is hence not what society would like to save out of an increase in income but what it would actually save (in a schedule sense) if that increase were to take place (while other variables remained unchanged). If we then divide investment into "desired" and "undesired" components, it follows that saving is identically equal to the sum of "*desired* investment" and "*undesired* investment." The "*undesired* investment" may be most easily

[8] Pp. 48-50. My suggestion of "little tangible to attack" referred specifically to my endowment of "equilibrium" with no more than an unstable and largely definitional character. Hamberg's interpretation of this to mean that I "confidently" considered my article or any part of it beyond scientific criticism does credit neither to his nor to my epistemological awareness.

[9] The qualification "at a given time" is, I believe, mine rather than Harrod's. It suggests perhaps a more appropriate criticism of the concept of an equilibrium rate of growth: granting that such a rate exists, may it vary capriciously from moment to moment with exogenous variables such as expectations, politics or the state of the weather?

[10] See R. F. Harrod, *Towards a Dynamic Economics* (London, 1948), p. 82.

understood if considered to be excess inventory accumulation.[11] It is "*desired* investment" which Hamberg must be talking about when he discusses investment functions and refers to "the equality of saving and investment in equilibrium."

It should hardly be necessary to assert, in the year XVII since the *General Theory*, that planned or desired investment and actual saving are not "functions of the same determinants." I might add that this assertion need not rule out the possibility that the level of desired investment does have *some* effect on the proportion of income saved. Thus there is no need to retract the thought implicit in footnote 10, p. 54, that the amount which businessmen want to invest may actually influence the proportion saved out of a given income. For example, this influence is highly significant in the case of corporate saving and investing, where the saving and investment may be in effect identically equal for the individual saver-investor. We might even (!) ascribe *some* role to the interest rate in making the proportion of income saved a function of desired investment (and hence, indirectly, a function of the existing stock of capital and productive capacity).

Definitions do not, of course, alter substance and we could reach the same conclusions as I have (perhaps with more difficulty[12]) by using Hamberg's definitions. Thus, we might state that by investment we mean Hamberg's desired investment, the magnitude of which is determined by the acceleration relation. We could argue further that the equality of saving and desired investment is the condition for continuance of any given rate of growth. (We would here be ignoring the problem of encouraging growth by making saving always less than desired investment.) Then, in order to maintain any particular rate of growth, the level of income and the proportion of underemployment must be such that the amount saved would equal the amount of desired investment. We could thus assert, as does my article, that the proportion of underemployment related to any given sustained rate of growth depends upon the functions determining the amount of saving and the productivity of saving. Or we might state, as does my article, that once the saving and productivity parameters are given, the underemployment ratio is determined by the rate of growth. The system may be summarized briefly as $f(\alpha, \beta, \sigma, Y, dY, P) = 0$; expression in this implicit fashion has the virtue of implying nothing about the causal relations of the parameters. The function can then be made explicit to bring to the fore whatever parameter we should like to make dependent or otherwise the focus of attention.

5. Hamberg's comments on what he interprets to be my *ex ante* saving function are, I believe, partly answered in section 4, above: the saving function or schedule in my model would be the same, *ex ante* and *ex post* (any

[11] These relationships are presented more fully in my "The Invariant Multiplier," *Rev. Econ. Stud.*, 1949-50, XVII (3), 198-202. See also Lloyd A. Metzler, "Three Lags in the Circular Flow of Income," in *Income, Employment and Public Policy* (New York, 1948), esp. pp. 18-20.

[12] As it stands, Hamberg's equation (21a) is not useful for describing the relationships between rates of growth and underemployment with which my article is concerned.

difference between *ex ante* saving and *ex post* saving or investment would flow from a difference between *ex ante* and *ex post* income rather than a difference in the schedules). However, I remain puzzled by criticism that I should make saving (investment) a function of the productive capacity-income ratio rather than productive capacity alone, only to note Hamberg's admission (fn. 18) that I have done just that.[13]

Hamberg's objection to my equation

$$S = \alpha Y - \beta P \tag{19}$$

may perhaps be dismissed by reminding the reader that he has been willing to accept an equation using the term "P/Y" (19b).
For (19) can easily be rewritten:

$$S = Y\left(\alpha - \beta\frac{P}{Y}\right) \tag{19'}$$

Then saving may be viewed as a function of income and of the ratio of productive capacity and income. Dividing through by Y, we have

$$\frac{S}{Y} = \alpha - \beta\frac{P}{Y} \tag{19''}$$

indicating that the proportion of income saved is a function of the ratio of productive capacity to income (nothing more than the reciprocal of my employment ratio).

While I see nothing "wrong" with equation (19), I have no objection to passing over it to equation (22) which makes more explicit the relation between saving and the employment ratio. My main purpose in introducing (19) was expositional, to offer a more gradual transition from the simple function where, as in the Domar model, saving is a constant proportion of income.

Hamberg's charge that equations (19) to (21) in effect retain the assumption that saving is a constant proportion of income reflects a serious methodological issue, perhaps compounded with an inadvertent repetition of Wagner's error. It would appear to me useful to distinguish between that portion of saving which is a function of income and those portions which can be significantly related to other variables. The hypothesis that saving (or consumption) is related only to income is clearly one of limited usefulness (as some of our recent ill-fated predictors of national income will certainly testify). However, Hamberg's statement that my equations (19) to (21), and by later inference (22) as well, leave the marginal and average propensities to save equal is true only under some *ceteris paribus* assumption which, in effect, relegates other variables (in this case, productive capacity or the employment ratio) to oblivion. Indeed, this may be accomplished most simply by subjecting the

[13] Hamberg seems to feel that he has made some significant change in my Y/P term by inverting it and changing the sign. Actually, in an abstract model of this kind, where the main content given to relationships goes no further than the signs of partial derivatives, a change such as this is of no substance; its chief effect that I can see is to take attention away from the underemployment (employment) ratio to which I tried to call attention by using the relationship in the form Y/P.

saving function to the constraint of the "equilibrium" rate of growth condition as did Wagner. Hamberg declares himself in agreement with my objection to Wagner on this point. Yet, without Wagner's (improper) assumption of equilibrium my saving functions will meet the test Hamberg prescribes in the beginning of his footnote 22. Thus if

$$S = \alpha Y - \beta P, \tag{19}$$

on the common two-dimensional graph which Hamberg has in mind for relating saving and income, we have indeed "a linear function which intercepts the S axis below the origin"; the S-intercept is, of course, $-\beta P$ (that is, when income is zero, saving equals $-\beta P$). With Wagner's assumption that $P = \frac{Y}{\theta}$ —an assumption that Hamberg rejects—this S-intercept assumes the value of zero (of course, if $P = \frac{Y}{\theta}$, when $Y = 0$, $P = 0$). It is difficult to see how Hamberg can both agree with my objection to Wagner's criticism and claim that I have retained saving functions which kept constant the proportion of income saved.

Robert Eisner*

* The author is assistant professor of economics at Northwestern University. He is indebted to C. Addison Hickman, Fritz Machlup, and Robert H. Strotz for helpful comments on a draft of this rejoinder.

N. Jasny's "The Soviet Economy during the Plan Era": A Correction

I regret the occurrence of a serious error in my review of this work.[1] Jasny's measurement of Soviet social accounts in terms of United States dollar prices of 1926-27, about which I had expressed certain misgivings, is not as heavily relied upon by the author as I had mistakenly assumed. It is, in fact, confined to his evaluation of the total *outputs* of various economic sectors, such as industry (broken down into consumers' and producers' goods), agriculture, and construction, and to an appraisal of investment outlay and its chief components. In each case a parallel valuation in terms of *Soviet* prices of 1926-27 is given, and the final structure of national income, as it emerges from Jasny's calculations, is entirely based on these prices.

Serious as this error may be, it does not, I think, materially affect the validity or relevance of my observations concerning the danger of using "esoteric" price structures in Soviet national income analysis. While Soviet prices of 1926-27[2] are not of course esoteric to the postwar Soviet economy in a *spatial* sense, they still suffer from a remoteness in time which is greatly aggravated by the intervening process of industrialization. In fact, the unprecedented violence of this process may well have rendered them quite as inapplicable as the prices of distant, but industrialized, America. Jasny is of course fully aware

[1] *Am. Econ. Rev.*, Mar. 1953, XLIII, 185-89.

[2] I refer here to the *actual* prices of that year as estimated by Jasny, and not to their conventional substitutes as used in Soviet statistics.

of these difficulties and his choice of the 1926-27 Soviet price pattern in preference to those of other years or other countries was not made without a good deal of critical examination, but the doubts which I ventured to express in my review concerned the general principle of such price imputations in the Soviet context compared with other methods of eliminating bias, where these can be used. Once the principle is conceded, Jasny must be deemed to have used the material available to him to the fullest advantage, subject to the qualifications concerning the estimation of composite price trends which I advanced in my review.

F. Seton

BOOK REVIEWS

Economic Theory; General Economics

Wesley Clair Mitchell, the Economic Scientist. Edited by ARTHUR F. BURNS. (New York: National Bureau of Economic Research. 1952. Pp. viii, 387. $4.00.)

One of the things that serves in a small way to offset the loss of an outstanding personality in any scientific field is the realization that his passing will probably be followed by the publication of a memorial volume. One can then look forward to the intellectual delight of contemplating the whole of an illustrious scientific career. This has now come to pass with respect to Wesley Clair Mitchell, for whom Professor Authur F. Burns has prepared this memorial volume. Part I of this book, entitled "Life and Work," includes a lengthy introductory sketch by the editor, a delightful personal sketch by the late Professor Mitchell's wife, and a number of professional sketches and memorial addresses. This material reveals to the outside world the unique qualities of Mitchell's personality, the feeling of lofty purpose which dominated his life's work, and the inspiration which he gave to all his associates.

The appraisals of Parts II and III are attractively introduced not by the original titles of the articles, book reviews, and chapters selected for inclusion in this volume, but by new titles chosen by the editor. The early appraisals deal with Mitchell's "Place in Contemporary Economic Thought" (P. T. Homan), his "Contributions to the Theory of Business Cycles" (J. M. Clark), and his "Views on the Scope and Method of Economics" (A. B. Wolfe). The recent appraisals cover Mitchell as an "Economic Theorist" (Milton Friedman), an "Economic Naturalist" (E. B. Wilson), an "Historian of Economic Thought" (T. W. Hutchison), a "Social Scientist and Social Counselor" (A. H. Hansen), and as a "General Economist" (J. A. Schumpeter). The limitations of space do not permit an extended discussion of these various appraisals of Mitchell's work. It may be pointed out, however, that, as far as the major appraisals are concerned, the pros and cons are equally divided. The short commentaries of E. B. Wilson and T. W. Hutchison, which are in the form of book reviews, while interesting do not raise any major issues. Favorably disposed toward Mitchell are Clark, Wolfe, and Friedman, while somewhat inclined in the opposite direction are Homan, Hansen, and Schumpeter. Following Burns' suggestion, Friedman develops the interesting thesis that Mitchell's cycle analysis can be readily restated in the modern vocabulary of propensities, multipliers, acceleration coefficients and the like. Unfortunately for his own side of the case Clark plays somewhat into the hands of the opposition by stating in his appraisal that Mitchell emphasizes the "how" rather than the "why" in his cycle analysis. Wolfe's excellent appraisal is the only one that draws attention to the integrative philosophy which lifts Mitchell's work far above the level of the ordinary.

Leading the opposition, Homan's facilely written appraisal says in effect that, if institutionalism were worth cultivating (which is not the case, Homan believes), Mitchell, being without originality and philosophical grasp (p. 190), was not the one for the job in hand. The highlight of Hansen's two-pronged appraisal is the assertion that Mitchell's deficiencies as a social scientist are carried over into his views as a social counselor. Hansen's position is that, since Mitchell largely ignored the investment-saving problem, as a social counselor he failed to appreciate the value of "monetary and fiscal policies, as possible stabilizing and expansionist factors operating upon a mixed society . . ." (p. 316). There will be those who do not accept Hansen's interesting argument, asserting that Mitchell's totalistic approach quite logically fostered similarly broad views in the field of economic reform. In his appraisal Schumpeter brings up the rear guard of the opposition with the observation that Mitchell was really an equilibrium economist in spite of himself, who unfortunately was dazzled by the glitter of what Schumpter calls the "dubious Veblenite gems." It is apparent that there is a wide intellectual gulf between Schumpeter and Mitchell reflecting the basic differences between their respective mentors, Eugen von Böhm-Bawerk and Thorstein Veblen.

The various aspects of Mitchell's work are respectively seen by the reader through only the eyes of the single economist whose appraisal has been selected for inclusion in this volume. For example, there are not two or three different economists' appraisals of Mitchell's "place in contemporary economic thought." This reviewer would be much happier if readers had been given an opportunity to consider Mitchell's place in the stream of economic thought from a variety of points of view. And the same criticism applies to all the other appraisals which deal with the various facets of Mitchell's work from the point of view of only one critic.

In his prefatory note Burns explains that he did not permit his scientific sympathies to influence his selection of appraisals. He made every effort to make room for the views held by representatives of different schools of thought. There are commentaries from institutionalists, Keynesians, marginalists, and others not so easily classified. But each topic is not appraised from the points of view of these different schools. A really effective treatment would have called for appraisals from different points of view of each of the aspects of Mitchell's work listed by the editor. The reader would then perhaps be in a better position to make up his mind about Mitchell's work and to arrive at that "just and balanced appreciation" of Mitchell which the editor says in his prefatory note he hopes the reader will achieve.

The over-all effect of these appraisals must be somewhat disconcerting to many readers. They learn from Burns that Mitchell was quite aware of the value of equilibrium price theory (p. 47), but from Schumpeter that Mitchell would throw this theory "overboard" (p. 328). They are told by Friedman that Mitchell had all the elements of modern cycle theory in his own analysis of the cycle (p. 271), but by Hansen that Mitchell was concerned with measurement and description but not explanation of the cycle, and also at times showed "no recognition or grappling with the problems of investment

and saving (p. 315)." The reader is also informed by Schumpeter that Mitchell "never advocated 'policies' " (p. 326), but by Hansen that Mitchell's policy recommendations were too "radical" and "drastic" (p. 317). Homan tells the reader that Mitchell did not have an original mind (p. 186), while Clark asserts that Mitchell's work was of the "formative type" (p. 193), which presented a significant challenge to orthodox theory. Anyone thoroughly acquainted with Mitchell's attainments will wend his way through this curious maze of contradictions without any trouble. But what of the great majority of readers into whose hands this volume will fall sooner or later? Are they to conclude that Mitchell was a chameleonlike character about whose color one could never be quite certain?

The eight appraisals in this volume make little mention of the deep unity which runs through all of Mitchell's scientific interests. He had a highly integrated personality which was solidly rooted in a clear-cut philosophical position. His pragmatic philosophy, drawn from a number of different sources and distilled in his essentially independent mind, shows up everywhere—in his scientific method, in his theory of human nature, in his views on the scope of economic science, in his grasp of the interrelations of the social sciences, and in his attitude towards economic reform. It seems to this reviewer that a balanced appreciation of Mitchell can be secured only if the reader has brought to his attention the ways in which Mitchell's philosophical position binds together all that he did. Acceptance or rejection of his philosophical position is not the issue here. It is, instead, how can one secure a satisfactory understanding or appreciation of Mitchell? The answer obviously is: only by uncovering the basic unity which nourished all of Mitchell's scientific interests, and by tracing this unity throughout his scientific attainments.

The unity in Mitchell's work is not difficult to uncover. It runs through his business cycle analysis, his theory of capitalism, and his analysis of the problems of economic welfare. What Mitchell set out to do in the field of economics is quite clear. He wanted to enlarge the area of economic analysis until it became a "comprehensive study of economic behavior." The older studies, by which Mitchell meant equilibrium economics centering in price theory, should be added to until a broader economics emerges. The first step in this widening process was the development of business cycle theory; the next step was the working out of a theory of capitalism. The third step was to show the public-policy implications that flowed from his theory of capitalism. This trinity of Mitchell's interests may be separated for purposes of critical analysis, but for that "just and balanced appreciation" of Mitchell to which Burns refers, it is necessary to consider the three portions of Mitchell's work as parts of a larger whole. It is true that this trinity of interests did not receive anything like a well-rounded treatment by Mitchell. In this volume Friedman picks out a full-fledged theory of the cycle from what he describes as Mitchell's business-cycle "grab bag." It remains for someone to piece together in a similar manner the mosaic of Mitchell's theory of capitalism. His totalistic theory of the economic order is only fleetingly referred to in this volume (pp. 37 and 71). How much richer and more valuable to the general reader this volume would

have been had it included an essay on Mitchell's theory of capitalism, partially developed though it was. And the same can be said of Mitchell's theory of economic reform.

How did Mitchell view his own work? The answer to this question is given in the correspondence published in this volume for the first time, in which Mitchell states that "what I think is worth while is to accomplish some of the necessary pioneer work toward the construction of useful economic theory" (p. 66). This pioneering attitude toward the tasks to be accomplished was both an asset and a liability. As an asset it led Mitchell to look beyond the confining boundaries of orthodox economic thought, and to follow paths which promised more light on the economic problems of the first half of the twentieth century. But, as a liability, this pioneering attitude led Mitchell to pay little attention to the finished constructions of the general treatise and the polished refinements of the technical theorist. Although there may be some support for the view that Mitchell remained too long a "pioneer," still there can be no doubt but that he has won for himself an important and enduring place in his science's hall of fame.

Allan G. Gruchy

University of Maryland

Einführung in die Wirtschaftstheorie. III. Teil, *Geld, Kredit, Volkseinkommen und Beschäftigung.* By Erich Schneider. (Tübingen: J. C. B. Mohr [Paul Siebeck]. 1952. Pp. vi, 220. DM 17.00; paper, DM 14.00.)

With the publication of the third volume of his *Einführung,* Professor Schneider has completed his endeavor to present to his readers "all tools of theoretical reasoning that are needed for an analysis of the economic realities around us" (p. iii). His work, in this reviewer's opinion, constitutes one of the most comprehensive, most up-to-date, and pedagogically most serviceable of modern texts in economic theory.

The first volume, entitled "Theory of the Circular Flow in the Economy," appeared in 1947 and was reviewed in the September, 1949 issue of this *Review.* The second volume, on "Economic Plans and Economic Equilibrium in the Market Economy," appeared in 1949 and was reviewed in the September, 1950 issue. The third volume is about "Money, Credit, National Income and Employment." It deals with "macro-economics" (p. 2) in contrast to the second volume, which dealt with "micro-economics"; and it presents *ex ante* analysis in contrast with the first volume, which was largely confined to *ex post* analysis. (Looking backward, this reviewer has some doubts about the necessity or expedience of devoting so much space to *ex post* analysis, and he wonders whether the author would not have arranged matters somewhat differently if he had known *ex ante* all that he now knows *ex post.*)

The present volume is divided into three chapters. The first is on "The Means of Payments in our Present-Day Economy"; it is only seven pages long. The second chapter is on "The Creation and Cancellation of Money" (80 pages); the third is on "The Determinants of National Income and Its Fluctuations" (123 pages).

Schneider is ultramodern in every part of his text. He is ruthless in his treatment, or rather nontreatment, of old-fashioned theories or obsolete institutions. That the chapter on Money and Banking does not include the traditional discussion of the silver standard and of bimetallism is probably all to the good; but there is not a word about the gold standard either. This neglect, I am willing to concede, of a currency system that no longer exists is admittedly preferable to the customary overemphasis on, and long-winded exposition of irrelevant details of, the gold standard. (Excessive attention to institutional detail has prevented many textbook writers from noticing that even the United States and Switzerland have long abandoned the gold standard.) But in a textbook a discussion of the "creation of money" that does not have anything to say about gold can hardly be considered complete. The word "gold" does not appear in the index; it does appear twice in the text—together with foreign exchange and securities—when the essentials of open-market policy are explained (pp. 54 and 73); but there is no discussion of purchases or sales of gold by the banks or monetary authorities, of possible effects of changes in the official price of gold, of restrictions in the gold market, of the consequences of gold hoarding.

One cannot say that this contempt of past, recent, and current history is due to any aversion to institutional "facts" on the part of the author. That he has no such aversion he shows, for example, by the two pages on the present German banking law which he includes in the exposition of the principles of multiple expansion of bank credit. When he omits something from his discussion, he obviously does so because he thinks it is not important. He makes this explicit in his reference to the quantity theory of money (which he does not regard as worthy of discussion in the analysis of money and banking, but reluctantly includes as a brief digression in the exposition of national income formation). "Velocity of circulation" is referred to with obvious scorn; transactions velocity of circulation of money is not mentioned anywhere in the text; income velocity is accorded one page, chiefly to prove its irrelevance on the ground that it is "no institutionally determined constant" (p. 168). I do not know why this should make it irrelevant; but, as a matter of fact, its upper limit is probably institutionally determined and, indeed, this ceiling velocity plays a significant, though only implicit, part in Schneider's very fine and elegant analysis of liquidity preference and the investment multiplier.

The various techniques of banking policy—reserve requirements, open-market policy, discount policy, official declarations—are satisfactorily treated; but there is no discussion of objectives or guides of monetary policy. The best part of the chapter on Money and Banking is the exposition of the coefficient of multiple expansion of bank credit as a function not only of reserve requirements but also of differential habits of payments. This is a model of clarity.

Liquidity preference is Schneider's favorite theory. After having given it several pages in Volume II, he treats it again in the present volume extensively in connection with open-market policy (pp. 57-72), with the determination of investment and income (pp. 140-60) and with the refutation of the quantity theory (pp. 164-66). Since he admits that the significance of liquidity

preference is confined to short-run income analysis, one wonders why he accords it such generous treatment. The prominent rôle assigned to this tool of analysis is perhaps partly attributable to the fact that Schneider has developed an especially elegant way of dealing with it verbally, geometrically, and algebraically.

Mention should be made of the variant form of the liquidity function that Schneider uses to exhibit the relationships between interest rate, income level, and money supply by curves expressing the relationships between the first two of these variables with the third as parameter. These curves show the combinations of interest rates (determining the size of speculative cash balances) and income levels (determining the size of transactions cash balances) that can be accommodated by fixed quantities of money. Schneider, following Hicks, draws such curves with horizontal and vertical ends, indicating that the interest-elasticity is infinite at low interest rates (because of unlimited absorption of speculative cash) and zero at high income levels. This zero elasticity—since Schneider wants income to be understood as real income or money income with constant prices (p. 97)—must be due to a ceiling velocity of circulation preventing any higher income levels from being financed with a given quantity of money. Schneider, at one point, attributes these vertical ends of the interest-income curves to the fact that "with a given amount of money a definite money income cannot be exceeded, for reasons of liquidity preference alone. Higher incomes are possible only with a larger quantity of money" (p. 148). At another point, however, he attributes the vertical end of such an interest-income curve to "full employment" (p. 149); but the effects of full employment would be more correctly described by a family of these interest-income curves converging on a single vertical stretch, indicating the fact that no further increase in the quantity of money can succeed in promoting an income level beyond the full-employment level.

Schneider's exposition of pre-Keynesian, Keynesian, and post-Keynesian multiplier theory is the most useful part of the book. The quotations from N. Johannsen's early formulations of that theory, in 1903 and 1913, are very interesting. In the presentation of the Keynesian apparatus Schneider shows himself as one of the master-teachers of our time. I should like to point especially to the discussion of the savings function (p. 116) and of the interdependence of Keynes' (supposedly independent) fundamental functions (pp. 139-47). In presenting these relationships Schneider takes advantage of the ingenious methods developed (in Swedish) by Tord Palander.[1] It is made very clear why the Keynesian multiplier formula fits only a special case, namely, the very special assumptions that the interest-elasticity of investment is zero or the interest-elasticity of liquidity preference is infinite. A general multiplier formula is presented, containing terms taking account of income-induced changes of the interest rate and of interest-induced changes of investment (p. 151).

Schneider earns the characterization of his text as the most modern and up-

[1] T. Palander, "Keynes' almänna teori och dess tillämpning inom ränte-, multiplikator- och pristeorien." *Ekonomisk Tidskrift*, 44, 1942, pp. 233 ff.

to-date when he includes such recent discussions as Haavelmo's "multiplier effect of a balanced budget" (pp. 198-202) or Patinkin's, Hansen's, and others' views on the "Pigou effect" of continuing price reductions (pp. 158-61). But, as I said before, Schneider is modern also in the problems which he chooses to omit. This reviewer is particularly troubled by the fact that a three-volume text on economic theory, by an author who on many issues is an intellectual descendant of Wicksell, should be completely without any discussion of, or even reference to, capital theory. Of course, the pure theory of capital or of the time structure of production is relevant chiefly to a long-run analysis of income, and this the author has explicitly excluded from the scope of his book (pp. 93 and 178). But is there a good excuse for such an exclusion from a three-volume treatise on economic theory?

At one point of the present volume Schneider tries, as he has in the first volume, to contrast *ex ante* and *ex post* conceptions in economic analysis. This is what he says: "If we know the consumption planned by households for a future period . . . and the net investment planned by business firms . . . , then we are able to say what the resulting income will be if the plans of the households and firms are realized" (p. 93). How true and how useless! Yet, this statement is regrettably typical of much of our modern theorizing. Too often do we forget that one of the foremost tasks of economics is to explain and predict circumstances under which consumption and investment plans cannot be realized. And it is my conviction that capital theory is needed precisely for that task.

These remarks may seem to contradict my appraisal of Schneider's work as one of the most comprehensive of modern texts in economic theory. But there is no contradiction since capital theory is now generally out of fashion and its neglect is common to all modern texts. Thus my criticism is not directed against Schneider but rather against current fashion in economic theory. Indeed, I expect that most readers will side against the critic and with the writers who have expelled from economic theory the most complicated part of economics. This old-fashioned reviewer dares to predict that capital theory will be called back from its exile, for the concern with the economic problems of underdeveloped countries will eventually demonstrate that the lack of capital can neither be made good nor more palatable by a lack of capital theory.

Fritz Machlup

The Johns Hopkins University

Economic Anthropology. By Melville J. Herskovits. (New York: Alfred A. Knopf. 1952. Pp. xiii, 547, xxiii. Text ed., $5.75.)

This book is a complete revision of the author's earlier work, which in the first edition (1940) was entitled *The Economic Life of Primitive Peoples*. Both the scope and analytical categories remain substantially the same. The principal changes consist of an introductory, theoretical chapter on "Economizing and Rational Behavior," a considerably expanded (though by no

means exhaustive) sampling of the relevant literature appearing within the intervening twelve years; expanded treatment of motivation and rewards for work (now two chapters instead of one); and a somewhat fuller treatment of the value concept and of the capitalization process in these societies. In this edition Herskovits also relates his general discussion and illustrative materials to a broader representation of economic theories.

As in the first edition, the "primary concern in these pages is to understand the cross-cultural implications of the process of economizing," with primary emphasis upon the "role of alternatives between which to choose . . . and the problem of attaining efficiency through choosing" (p. 4). Cultural factors, it is stressed, delimit not only the choosing but also the nature of wants themselves and the exchange system for the provision of goods and services.

It is apparent that, from the nature of the societies which he conventionally studies, the anthropologist finds the approach of institutional economists more congenial to his material. A perusal of the literature, for example, reveals the significance of gift and ceremonial exchange as the primary mechanism of circulating goods within a group. While the economic rôle of these processes can be readily traced in terms of magnitudes, frequency of occurrence, and personnel involved, the forces that determine their operation are mainly noneconomic: kinship, neighborliness, fear of sorcery, marital ties, etc. The concept of equilibrium, therefore, would have to be related not merely to the ratio of income to outgo in the society as a whole, but also to "motivation and the balance felt by the individual in his functioning capacity as a member of society" (p. 173).

Comparative study of economic behavior based upon peasant and primitive type societies poses certain difficulties for the economist, who might wish to draw upon a wider empirical base in support of his principles and propositions. Perhaps the foremost of these is the fact that such societies generally constitute small groups in which the intimacy of interpersonal relations is widespread. Kinship and familial reciprocities flavor all areas of activity. In addition to material inducements, incentives to work, for example, are caught up in varying patterns of mutual responsibilities based on these and related factors. Individuals discharge their responsibilities consistently as much in accordance with attitudes toward social (nonrational) as with those toward economic (rational) efficiency.

In the same way that the comparative analyses of cultures and societies by anthropologists contribute principally to sociological theories of small group behavior as compared with mass behavior, economic anthropology might be expected to contribute to middle-level hypotheses geared to the context of such groups. Propositions of this kind and the conceptual apparatus upon which they are based should be very useful for the empirical study of western economic behavior and institutions at the community level. Indeed it is only at the upper range of technological and societal complexity among folk-type societies that we encounter something like a classical economic system. Thus among the Panajachael of Guatemala we find local specialization in a competi-

tive market, buying and selling for cash, prices geared to supply and demand, with rates made on the basis of cost estimates (p. 235). Capital formation here as elsewhere, however, is minimal and does not occur in accordance with values of growth or progress.

The author has chosen to limit the parameters of his analysis so as to exclude the processes of change. This is perfectly permissible, but as he has adopted the title, *Economic Anthropology,* the reader might feel entitled to learn something from the considerable literature on changes in process among folk and primitive societies consequent upon the diffusion of Euro-American technology and socio-economic values. This omission is all the more lamentable in view of the extremely important increase in interest on the part of economists in theoretical problems of growth and development.

As a final comment, this reviewer would like to suggest as perhaps a more felicitous organizational framework for such a volume, a chapter or two pulling together theoretical considerations and problems discussed throughout, followed by a series of rather full case studies of nonwestern economic systems. The considerable range of variation among so-called primitive societies, as regards degrees of isolation, complexity, population size and density, makes this approach desirable, as well as more amenable to critical analysis by the student, than does the juxtaposition of isolated examples of bits of behavior in support of widely scattered general statements. There is presumably a high degree of interdependence of the parts of any such system. A good model for a brief introductory section as envisaged here is a splendid chapter by R. Firth on "The Social Framework of Economic Organization" in a recent volume entitled *Elements of Social Organization* (1951).

Like its predecessor the volume in review will be of primary value to the economist in demonstrating " . . . some of the discrepancies between [his] assumptions about non-pecuniary, non-machine economies and the actualities of primitive production, exchange and consumption."[1]

BERNARD J. SIEGEL

Stanford University

Economics: Trends and Issues—A Book of Readings. Edited by SCOTT KEYES. (New York: Russell F. Moore Co. 1952. Pp. ix, 508. Paper, $2.90.)

The present collection of article reprints, intended as supplementary reading for the beginning student, enters its struggle for market survival in competition with recent collections by Gayer, Harriss, and Spencer (1951); Hess, Gallman, Rice, and Stern (1951); Isaacs, McKee, and Slesinger (1952); and Samuelson, Bishop, and Coleman (1952)—among others. It is a rare anthology that pleases all, and there is always the temptation (in the absence of some solicitous publisher who would issue separately bound individual articles) for each economics department to compile its own "best" offering.

[1] From a review of the first edition by C. M. Arensberg, *Am. Econ. Rev.*, Sept., 1940, XXX, 595.

Keyes's book contains an impressive list of contemporary contributors, drawn with few exceptions from the ranks of professional economists. The articles give a beginning student an excellent sampling of many current economic problems and the techniques used by those working on them.

Part I is the usual sort of introductory section on the scope, method, and need for economics. Aside from an informative excerpt from the 1949 Report of the Council of Economic Advisers, there is an unusually well-written and appealing lead article entitled "It's Political (Repeat Political) Economy." The book's long second part is an exposition of "Basic Characteristics of the American Economy," and features such authorities as A. A. Berle on industrial concentration, Hauser on Census trends, and Leontief on the cost-price structure. Methods of economic measurement are explored further in the third section, with Commissioner Clague holding forth on the Bureau of Labor Statistics Consumers' Price Index, and Richard Ruggles contributing a most useful chapter on national income accounting in relation to policy decision-making.

Part IV covers Keynesian economics and the sort of model-building and forecasting attempts to which it has given rise. Gerhard Colm discusses the Nation's Economic Budget, and there are illustrations of the current investigations by the Survey Research Center and by the Department of Commerce into determinants of consumer and investor behavior. Various policy problems are considered at some length in Part V, with the authors ranging from the members of a recent American Economic Association subcommittee on economic instability to Paul Douglas on the welfare state and Gordon Gray on foreign economic policy. A final section presents, all too briefly, a potpourri of J. M. Clark, Polanyi, and Schumpeter on economic thought, economic development, and comparative systems. The section concludes with the United Nations comparison of seventy countries' national and per capita incomes which, even after severe statistical qualification, retains a powerful impact.

In his preface, the author claims four major virtues for his book as compared with similar compilations. First, all the articles included date from the post-World War II period, with many only a year or two old. This immediacy of the writings may well give students an "up-to-date" view of economic problems and policies, but if not offset by classroom references to earlier works might equally well lead to an impression that all worth-while thoughts in our venerable science have been uttered only in the last few years. Those teachers, therefore, who desire to instill some acquaintance also with earlier giants of the field will not consider this distinctive feature an unmixed blessing.

A second distinguishing characteristic, suggested earlier, is that all the selections are from professional (including governmental) sources. This approach has resulted in a consistently high caliber of performance which ought to please economists. But again one might question why students should not also have been exposed to some other sources, ranging from Marxist tracts to National Association of Manufacturers releases, such as they may or should debate in their own everyday lives. The third point, that almost all the articles are presented in uncut form, does seem an undeniable virtue. About half the

thirty-five articles (far fewer than in other compilations) run from 10 to 15 pages, avoiding the usual pitfalls of excessive length or fragmentation.

Finally, the material has been selected with especial emphasis on means of measuring, and actual appraisals of, the changing developments of the past two decades. This approach produces a slighting of relative-price and allocation problems to a degree probably greater than is merited. In this sense the volume would serve more usefully as a supplement to introductory courses in income and employment than to those in price theory.

On the whole, though, it seems a virtue of this work in comparison with others of its sort that it does not dilute its coverage in an attempt to be all things to all courses. While the need for books which link beginning students with contemporary problems is even more pressing (because more difficult) in the fields of price, allocation, and market theory, within the scope he sets for himself Professor Keyes has given us a most useful educational tool.

DAVID N. MILSTEIN

Stanford University

The School of Salamanca: Readings in Spanish Monetary Theory, 1544-1605. By MARJORIE GRICE-HUTCHINSON. (New York: Oxford University Press. 1952. Pp. xii, 134. $2.50.)

The author of this little volume holds that Spanish writers have a claim to recognition in the development of economic thought which has been overlooked. She believes that this is especially true of the sixteenth and seventeenth century scholars identified with the University of Salamanca. In her appraisal of this group, Professor Grice-Hutchinson concludes that they made original and useful contributions in three important respects: a psychological theory of value applied to both goods and money, the quantity theory of money, and a theory of foreign exchange closely resembling the modern purchasing-power parity theory (pp. 47-48, 58).

The translations from original texts confirm her thesis that members of the School developed their idea of value around the concept of scarcity and utility (pp. 48, 86). Domingo de Soto set the pace when he declared that "the price of goods is not determined by the wish or convenience of individuals, but by the community," and added "that is by common estimation" (p. 83). He concluded that "prices rise when buyers are numerous and fall when they are scarce. Likewise, prices fall when sellers are numerous and rise when they are scarce" (p. 86). In a carefully documented chapter the author traces the influence of the Spanish writers through Grotius, Pufendorf, and Hutcheson, but fails to connect the statements on utility and rarity of Galiani, Condillac, and Turgot with the Spanish writers.

In the opening chapter, the author says, "the whole Spanish nation had learned by the middle of the sixteenth century that the value of money is fickle and that gold and silver are not synonymous with wealth" (p. 1). She contends that prices rose first in Spain, and that Spanish writers were first to note and trace the causes of the price revolution. Martin de Azpilcuetta

Navarro noted the influence of gold and silver on prices in Spain and applied the theory of demand and supply to money. His exact statement is, "And even in Spain, in times when money was scarcer, saleable goods and labor were given for very much less than after the discovery of the Indies, which flooded the country with gold and silver. The reason for this is that money is worth more where and when it is scarce than where and when it is abundant" (p. 95). Tomás de Mercado, however, seems to have the typical mercantilistic outlook (p. 96). In view of the mercantilistic legislation in Spain and the prevalence of mercantilistic tracts cited by Earl J. Hamilton in his studies of Spanish prices and Spanish mercantilism, some question remains as to how well the Spanish had learned the lesson.

Professor Grice-Hutchinson asserts that Navarro was the first writer to mention the influence of the imports of American gold and silver on Spanish prices and that his analysis of the quantity theory of money anticipated Bodin by twelve years. A comparison of the two writers reveals that Bodin was more thorough and there is little doubt that it was through the work of Bodin that the concept entered the field of economic literature.

The most original contribution of the School of Salamanca centered around their explanation of the variation of the exchanges. Here they definitely connected the price levels of the countries with favorable or unfavorable rates of exchange. While their version of the purchasing power parity was interesting and useful in relation to the question of exchanges and usury, any direct influence on later advocates of this theory came through Malynes and Mun rather than the Spanish writers.

In providing an English translation of these early Spanish tracts on monetary thinking, Professor Grice-Hutchinson has filled a gap in this chapter of the history of economic thought. Teachers and students will welcome her careful and lucid appraisals of the School as well as the availability of the original texts.

Rector R. Hardin

University of Georgia

The Development of Economic Thought. Edited by Henry William Spiegel. (New York: John Wiley & Sons. London: Chapman & Hall Ltd. 1952. Pp. xii, 811. $6.50.)

The history of economic thought abounds in good texts, long and short, analytical and descriptive, topical and chronological. There are also useful books of readings, masterworks, and compilations of short selections from the important writings of great economists. The volume under review is distinctly different. It is a history of economic criticism in the form of a collection of appraisals of the contributions of outstanding economists by later economists. In short, the book contains many of the best statements about the most important figures in the history of economic thought.

This volume by Professor Spiegel, therefore, supplements both texts and readings in its field by providing additional material of high quality in acces-

sible form. It gives the student better perspective by substituting the varied viewpoints of many observers for that of one scholar. It permits him to see how important economists have been regarded by different competent critics, and to discover how well they have met the issues of their day and have stood the test of time. The "craftsmanship" approach to this history of economic criticism is as unique as it is valuable and stimulating.

The book, consequently, should become popular in courses in the history of economic thought. Brief, helpful editorial notes introduce the various selections, some of which are familiar classics but others of which are new translations by the author of less familiar articles. The volume has a succinct foreword by Kenneth Boulding; there is also an explanatory preface by the editor. The index seems adequate. As will be shown, the coverage of the book is wide, and its organization is excellent.

The Dawn of Economic Science (the first part) gives Aristotle on Plato, Tawney on medieval economic thought, Heckscher on Mercantilism, Jevons on Cantillon, Smith on the Physiocrats, and Spiegel's own translations of Einaudi on Galiani and of Marx on the Physiocrats.

The Classical School (the second part) contains the article by Paul Douglas on Smith, those by Bonar, Fay and Keynes on Malthus, by McCulloch and Marshall on Ricardo, that by Mill on Bentham, by Viner on Bentham and Mill, by Cairnes on Bastiat, and by List on Say.

Socialists and Reformers (the third part) is rather short and seems insufficient. It includes articles by Halévy on Sismondi (editor's translation), Foxwell on Ricardian Socialists, Cole on Owen, Veblen on Marx, Hobson on George, and Tawney on the Webbs.

Historical and Institutional Approaches (the fourth part) is short but adequate. It contains Schmoller on Roscher (editor's translation), Mitchell on Veblen, Perlman on Commons, and Arthur F. Burns on Mitchell.

The Rise of Marginalism (the fifth part) is longer and even more valuable. It contains articles by Schneider on Thünen, Fisher on Cournot, Walras on Gossen (editor's translation), Keynes on Jevons, Hayek on Menger and Wiesser (editor's translation), Schumpeter on Böhm-Bawerk (editor's translation), Hicks on Walras, and J. M. Clark on J. B. Clark (previously unpublished article).

The Growth of Modern Economics (the sixth and final part) is short but helpful; it is naturally the most controversial as to what to include and what to exclude. It contains articles by Arthur L. Bowley on Edgeworth, Demaria on Pareto (editor's translation), Frisch on Wicksell (new article), Robbins on Wicksteed, Viner on Marshall, Haberler on Schumpeter, Samuelson on Keynes, and Colin Clark on Pigou (previously unpublished article).

The reviewer has little fault to find with what has been included in this collection of criticisms in the history of economic thought. His chief lament is the omission of important writers and entire schools of thought. The omitted schools include the Idealists or Moralists (except for Halévy on Sismondi and Hobson on George). With regard to individual writers, a few pages might well have been given to Marion Bowley on Nassau Senior. The obvious answer to

the criticism that whole schools of thought have been omitted is, of course, the limitations of space. Also, time has dimmed the importance of such once-famous optimists as Carey and his followers, even in America.

S. HOWARD PATTERSON

University of Pennsylvania

Economic History; National Economies; Economic Development

Trade and Industry in the Middle Ages. Edited by M. POSTAN and E. E. RICH. Cambridge Economic History of Europe, Vol. II. (New York; Cambridge University Press. 1952. Pp. xvi, 604. $9.00.)

Volume I of this series, published in 1941 and dealing with medieval agrarian life, announced in its preface that Volume II would be "urban, industrial and commercial." The general design was for an economic history of Europe, "or of the world as it impinges on Europe," starting formally from the later centuries of the Roman Empire. War and its aftermath removed contributors and cut off areas beyond any normal expectations. The schism of Europe concentrated upon the western part an emphasis which was planned for the whole continent and is particularly unfortunate for a discussion of industry and trade. Urban problems, defined as the growth, economic policies and finances of towns, together with "the history and policy of craft gilds" are now put over to Volume III, already slated to deal with medieval credit, finance, coinage, prices, state economy and economic thought.

In thus modifying a plan which had the advantage of corresponding roughly with university syllabuses, textbooks and the specialization of research, the new editors state that the earlier plan would have overloaded the second volume and was no more logical than the substitute. They admit that "background" topics such as prices, population and economic trends should be incorporated, not put off to the final volume on the medieval period. What this chiefly means in practice is their inclusion in Postan's tremendous (132 pages) chapter on medieval North-European trade. The reviewer wonders if this was the reason for placing his chapter before Lopez's chapter on medieval South-European trade. Considering his position in the craft, the rounded statement of Postman's interpretation may well compensate for more detailed chapters unavoidably lost.

However, a volume which is even more west-European in outlook than the first makes a notable concession with respect to the temporal starting point. An opening chapter by Gordon Childe discusses the industry and trade of "barbarian Europe" through stone, bronze and two iron "stages," down to Roman times. Like Heichelheim years ago, he sketches the distribution over great distances of products of basic industry—such as flint and obsidian points and edges—before written records. He also stresses the importance for economic development of trade in ornaments and other luxuries. This position seems difficult to harmonize with Postan's.

Chapters on Rome and Byzantium by Wilbank and Runciman are inter-

esting and informing, but seem to the reviewer too specific for a work of general synthesis. There is little to suggest the perennial movements out of Asia or a westward movement of settled life along both sides of the Mediterranean which was far from complete when the Roman Empire in the West broke up. That the Romans occupied only about half of the Atlas highland region of Africa and little of the Sahara is a fact with large reverberations in medieval and modern history. The way in which the Moslems completed the occupation, trade relationships between Spain and Africa during the great development of Moorish intensive agriculture, and the whole problem of Africa as a "granary" of Europe from Caesar Augustus to Lebrun and Daladier have surely "impinged" upon Europe more than incidentally.

The figure of 10,000,000 bushels for Rome's grain imports from Africa (p. 47) rests on no substantial evidence, and tends to distort the problems of population and food supply. Victor Demontès arrived at it in 1925, Tenney Frank in 1940, by putting together casual statements of Flavius Josephus and Aurelius Victor, made three centuries apart. Frank seems to have been aware that it is absurdly large for the number of mouths. Close students of the African area (roughly present-day Tunisia and the Department of Constantine) are roughly agreed nowadays on a maximum of some 3,000,000 bushels, and accompany any such figure with the warning that most crops in this erratic climate must have afforded less, some no exportable surplus at all.

One gets the impression of a more populous Roman Gaul from this volume than from the first. A treatment of thinly documented commerce over a long period has an almost unavoidable tendency to yield an exaggerated picture. Statements of absolute quantities in discussing earlier societies have the opposite tendency, whether comparisons with our time are made or left to the reader. Several of these authors quite properly insist that the volume of trade is a function of a particular society and situation, and is best not mentioned out of its setting.

Postan discusses the problems of quantities and records of quantities in medieval North-European trade after noting the character of the commodities exchanged and their approximate order of importance. His emphasis on interregional trade in staples is no greater than, for example, Rörig's in his 1933 *Mittelalterliche Weltwirtschaft*, but Postan's argument has another facet. Insistence upon the predominantly staple character of trade within medieval northern or transalpine Europe is made to suggest the relative independence or separate development of the area, and hence by implication to combat any notion of a dominance-and-dependency relationship with the Mediterranean. Rörig, and back of him others including Sieveking and Davidsohn, have noted the importance of staple foodstuffs, textiles, raw materials and other nonluxury items in Mediterranean trade, as does Lopez in this volume. Moreover, as Schulte, Rörig, Braudel and others have explained, staples were fairly important in the interregional trade between the Mediterranean and the North. Finally, as the various societies were organized, trade in luxuries was not necessarily unimportant or even indecisive for development, as Childe has noted in this volume. Consistently with his position on staples, Postan is

allergic to explanations of development as responding to changes in the supply of monetary metals or in prices. He is careful (*e.g.*, p. 166) to note that he is not arguing against such explanations as applied to later centuries. Like Rörig and others, including Usher, he stresses the importance of frontiers in medieval economic development, and notes that Hanseatic trade was roughly at its peak when the League was founded.

The main innovation in Postan's version of Hanseatic policy as defensive rather than progressive is his correlation of German history with a long and general contraction or depression which scholars have stressed in varying degrees of late years. Roughly speaking, the Black Death of about 1347-1350 marks the onset of this period, though recurrent epidemics, the Hundred Years' War and other struggles of the time figure prominently in the asserted decline. Note that the Hanseatic League is dated roughly from the 1360's. Postan argues that Europe's population, cultivated area, production in general and trade all declined for something like a century, following a much longer period of growth. The prevalence of strong sentences about decay and weak sentences about growth in this immense and generally moving argument made at least one reader slightly uneasy.

Lopez has a much more restricted and moderate version of this interminable depression in discussing the Mediterranean area. Nef, in a chapter on mining and metallurgy which has a remarkably sustained continuity, would settle for about the same population in 1350 and in 1450. It may be recalled that Josiah Cox Russell (*Speculum*, 1945) believed in a decline of population from about 1348 to 1430, the latter time roughly marking a turning point. He relied mainly on demographic statistics. Postan presents a great variety of evidence, which is quite proper, but the reviewer got an impression that the meager population figures tended to disappear in the mixture. Consistent with its presentation of development in northern Europe as roughly equal and step by step with that in the Mediterranean region, Postan's chapter seeks to rescue medieval England within northern Europe from the relatively backward position which it has occupied in general accounts. This volume has no continental authors, as compared with twelve in the first.

In the other nuclear chapter, dealing with medieval trade in the South, Lopez has taken over less of the general subject-matter left homeless by failures of expected contributions to materialize. Considering his learning and his many revisionist studies, he here displays notable restraint, often retreating to sample cases on the ground that wider generalizations would be unsafe. Among the many revisions in this chapter, perhaps the most startling is that the fourth crusade was not diverted to Constantinople, but was so planned from the outset (p. 311, n.). The opening of the Black Sea to Latin traders was significant, but the crusade as such was a failure because Asia Minor was not taken. Two attempts to make Damietta in the Nile delta a Latin bridgehead failed, and the evacuation of the Holy Land for trading outposts on Cyprus and in Cilician Armenia was only a question of time. The only real disaster for the West resulted from a Papal ban on trade with Egypt, which the Catalans ignored, probably harming Venice and Genoa more than they helped themselves. Italian

business organization at the zenith of commerce and capitalism (his terminology), from the late 11th to the early 14th century, is discussed with tantalizing brevity. The great Italian diaspora by sea and land is placed in its historical setting. Having objected on page 338 to Pirenne's term "saturation" to characterize the crisis beginning around 1350, Lopez proceeds on page 344 to state almost exactly what people using the term have meant by it: in this case the end of frontiers and the tribulations of business organized on the assumption of continuing expansion but facing the reality of too many competitors for available markets. Like other authors of this volume, he thus explains a tendency toward defensive and regulative organization.

One slightly confusing feature of this volume is the variety of "middle ages." To Lopez, the period ends with the fall of Constantinople and the end of the Hundred Years' War in 1453. Postan generally carries it to about 1500. One of Nef's ten subdivisions is entitled "The Boom in Mining and Metallurgy, 1460-1530." Nef's chapter is itself a gold mine and hard to sample. The Romans, often accused of being unoriginal as engineers, are exonerated. They had quite deep mine shafts, as much as 600 feet, and their mechanical equipment as well as their processes were more advanced than any afterward to the 14th century. Nef makes one of the most interesting comments—amounting to a "saturation" theory—on the "great depression" of the latter middle ages. A tremendous and sustained medieval extension of settlement opened vast mineral deposits near the surface. Uses multiplied. In the end, shallow deposits began playing out much more rapidly than methods and equipment for deeper mining were developed. Nef's discussion of the generally free status of medieval miners, as compared with slavery and low esteem in ancient times, is most interesting. Another subject which he divests of countless illusions is that of regalian subsurface rights.

But the most informing chapter on medieval human relationships within a profession is that of Jones on building in stone. Somewhat like the merchant trading at a distance, the master or other mason typically operated largely outside his home area. Hence he was unusually free from local regulations. The prevalence of wages, and of even quite large-scale contracting, have been recognized especially since the author and Mrs. Knoop published their 1933 study on the economic history of English stone building in late medieval and early modern times. This work got long and enthusiastic reviews on the Continent. Jointly and individually, its authors have written much on the subject, but nothing which quite renders the service of this relatively brief synthesis.

Chapter 6, by Eleanora Carus-Wilson, on the woolen industry from the Romans to the triumph of English cloth is an illuminating discussion of a key subject. In the area around Flanders, the industry took hold because of the excellent wool and the situation for trade. Soon it outgrew the local supply of wool. English wool was the best, but there were competitors for it, clothiers in England itself eventually taking the major part and with it the market for cloth. Everywhere there was some danger that mere craftsmen would become organized, force participation in public affairs, and perhaps even get control of city governments. Only in Flanders did this occur on a large scale, and it

occurred at a time when the goose that laid the golden eggs was itself struggling for survival. The author has stuck closer to woolens than Rörig did to fustians, in an account which is vastly broader in effect than one industry together with the trade in its raw materials and product.

The extent and quality of the revisions presumably measure the importance of a volume such as this, consisting of syntheses by ranking experts. This book is well written, but not for babes—it is highly condensed and presupposes either some knowledge of history and geography or access to reference works. The advanced students, including teachers, to whom it is addressed first of all, will disagree with some judgments expressed. The reviewer thinks that some distortion has resulted from placing the chapter on northern Europe before that on southern Europe. Some factual statements may be questioned—not very many, I think, for it is competent work. The volume will be read, and will influence beliefs and interpretations, for many years. This is my excuse for writing so many words about it, knowing full well that it must be read in order to gain any sound appreciation of it.

M. M. Knight

University of California
Berkeley

The Browns of Providence Plantations: Colonial Years. By James B. Hedges. (Cambridge: Harvard University Press. 1952. Pp. xviii, 379. $6.00.)

This book, the first of a projected three-volume history of the family, carries the story of the Browns of Providence Plantations from the date of the first ledger entry by Captain James Brown on October 9, 1723, down to the ratification of the federal constitution by Rhode Island in 1790. In tracing the business activities of the family through these years, the author has drawn upon an unusually complete collection of letters, journals, receipts, invoices, and contracts. The result is a significant contribution to our understanding of the colonial business man; one that compares favorably with William T. Baxter's study of *The House of Hancock* or V. D. Harrington's book on *The New York Merchant on the Eve of the Revolution.*

Like most of their contemporaries among the colonial merchants, the Browns were ready to consider almost any venture that gave promise of financial reward. This led them to engage in such diverse activities as trading along the Atlantic and Gulf coasts and with the West Indies, hauling slaves from Africa, writing marine insurance, trading with England, sending out whaling expeditions, trafficking with the enemy, blockade running, and privateering. But the Browns were more than merchant shipowners. They started grinding cocoa beans as early as 1752, their spermaceti candle works were in operation in 1753, and in 1755, they took the first steps leading to the construction of an iron manufactory that began turning out pig iron and hollow ware a year later. In fact, by 1765, ". . . the Browns had become manufacturers first and merchants second. When capital was available for business expansion, it went not into a larger merchant fleet but into another branch of manufacturing" (p. 123).

These various activities are described in considerable detail and much new information is made available, especially in the chapters on the manufacture and distribution of candles and iron products. The author brings out very clearly how the diverse parts of the business were all integrated into a well functioning administrative unit. Multiple operations were in themselves insurance against risks that attended almost every phase of operations. The Browns were skillful in adapting their business to circumstances that changed frequently and sometimes violently.

According to the author, the members of the family were not endowed with any uncanny ability to foresee the future turn of events. "The secret of their long-sustained success lay primarily in the personal qualities and business methods of the family" (p. 329). Careful planning went into providing leadership from one generation to the next. "A younger brother, a son, or a nephew well trained by his elders was always ready to assume the responsibilities of leadership at the proper time" (p. 329). Interest in perpetuating a family business prompted a subordination of short-term profits to a long-run view of operations. No important move was undertaken without careful planning, be it a project to corner the tobacco supply for shipment to Surinam, or a decision to equip a privateer. These same considerations apparently account for the fact that the Browns were seldom innovators in the strict meaning of the term. Obadiah Brown in 1736 was the first Providence merchant to engage in the slave trade, but this was long after the Newport captains had shown the way. The Browns were not the first to manufacture spermaceti candles in the new world and they drew heavily on the experience of Pennsylvania iron producers in the construction and operation of their own furnace.

These points are well made and emerge logically from the more detailed accounts of the business. There are, even so, some aspects of the family's policies and practices that are not entirely clear. Why, for example, did the Browns trade with the Dutch rather than the British West Indies? Why were their first connections in England with merchants in Liverpool and Bristol and not London? Was this, as one reviewer has suggested, because the Browns did not belong to the elite class of colonial merchants, came from Providence and not from New Port or Boston, and lacked proper social and governmental connections?[1] Would missing records or a closer examination of existing documents give a clearer picture of the business arrangements within the family and with outsiders on particular ventures? Is the impression gathered from the narrative correct, that at no time after 1723 was the raising of capital funds a limiting factor on expanding operations? Could such funds always be provided from within the business or when the Browns joined with others on particular projects was it to obtain financial support and not simply a measure designed to distribute risks?

But the raising of these questions is more of a tribute than a criticism of a study that by its excellence makes us eager for others of equal quality.

Harold F. Williamson

Northwestern University

[1] Bernard Bailyn, "Hedges' Browns: Some Thoughts on New England Merchants in the Colonial Period." *Explorations in Entrepreneurial History*, May 15, 1952, IV, 229-83.

Studies in British Financial Policy, 1914-25. By E. Victor Morgan. (New York and London: Macmillan. 1952. Pp. xii, 380. $6.50.)

Of interest primarily to the specialist in British finance, these studies are also of interest to the general student of "defense economics." They tell the much-told tale of the great changes that came over the British financial economy from the outbreak of World War I to the return to gold in 1925, but tell it with the help of previously unavailable data and the better analytical tools that are a legacy of the "Keynesian revolution."

The book is divided into four parts. Part I, "Preliminaries," describes the financial crisis following the outbreak of war and the emergency measures taken to combat it; the growth and abandonment of "direct" wartime controls; and general movements of economic activity over the entire period. Part II, "Government Finance," deals with expenditure, revenue, borrowing and conversion operations, the changing structure and distribution of the national debt, and movements of interest rates. Part III, "Money, Credit and Prices," describes the wartime currency arrangements, and the activities of the Bank of England, the money market firms, and the joint-stock banks. The chapter describing price movements includes a useful analysis of the "mechanism of price changes" along modified Keynesian lines. Part IV, "External Problems," analyzes the balance of payments on current and capital account, external wartime borrowing and lending, and exchange rates. A concluding chapter summarizes the findings of the studies as a whole.

The presentation and derivation of statistical series and estimates occupy a very large number of pages. Only the hardy explorer of the jungle that is British financial statistics will find these fascinating. One can only see the importance of the book by reference to its final chapter. Yet there, as well as tucked in among the statistics, are judicious criticisms of financial policy and generally well-balanced judgments of the importance of various causal factors in the inflation, the deflation, and the mid-'twenties slump.

The author's main conclusions can be briefly summarized. "Budgetary policy was the major cause of the steep price rise of 1915-17 and of the controlled inflation which continued during the closing phases of the war." (It is evidence of the progress made in the theory of wartime finance that only a generation ago the British government thought it "sound" wartime finance to impose new taxation only sufficient to meet the rising interest charges and a sinking fund on the increased national debt.) Fiscal policy is seen as an *active* inflationary force only up to mid-1917, however; thereafter "the deficit played the role not of creating new demand, but of sustaining the Government's existing purchases in the face of rising costs." During this latter period Morgan considers the main weakness to have been uncontrolled wages.

Monetary policy, while fumbling, is seen as only a minor villain in the wartime inflation. "It is one of the main theses of these studies that money and credit exercised a permissive rather than a causal influence." The Bank of England was hampered in its monetary control by an influx of gold and "automatic" issue of currency notes and Ways and Means Advances to the Treasury. But, in the main, technical devices and Bank-Treasury co-operation overcame

these difficulties. There is no suggestion that monetary policy could have done much more to prevent inflation.

In the brief postwar boom, Morgan finds, "Government finance . . . ceased to have any directly inflationary effect." Direct controls were relaxed too soon. Interest rates, though very high, were perhaps not raised high soon enough. But the boom was mainly brought about by rising consumption expenditures, a burst of private investment, and a sharp change to a large favorable balance on current international account, conditions largely beyond practical control by monetary policy.

Morgan criticizes most severely the handling of the slump of 1920-21, and the semislump of 1922-25. Substantial budgetary surpluses aggravated the malady, as did the maintenance of high Bank and market interest rates in 1920-21. "The extreme eagerness" to reduce the short-term debt by funding operations, an attitude based on "a widespread misunderstanding of the dangers associated with the floating debt" was especially injurious. "Once the boom had broken, spending required not restraint but stimulus." A more liquid debt structure and further broadening of the cash base "would have done nothing but good."

Morgan finds little evidence, on the other hand, for the view that monetary policies aimed at a return to gold at prewar parity were a serious depressing influence in 1924-25. "Both deposits and advances were maintained at high levels and short-term rates were moderate, while the fact that long-term rates were higher than before the war was due to conversion operations rather than banking policy." Here, it seems, Morgan has failed to give proper emphasis to the *trade* disturbances caused by the rise of sterling. He rightly points out that the wartime destruction of Britain's short-term creditor position, and the worsening of her general trade position, were serious difficulties regardless of the level of sterling. But this further impairment of Britain's competitive position, plus the greater measure of domestic monetary and fiscal austerity required to achieve and maintain prewar sterling parity, certainly aggravated these difficulties.

Morgan's gifts to the statistical jungle-fighter are substantial. He has obtained from the Treasury and the Bank of England hitherto missing data on such important matters as: the operations of the "British Treasury Account in New York" (through which was channeled a large part of Britain's external war finance); British wartime loan transactions with belligerents and neutrals; and wartime gold production and management. These hitherto secretive institutions also provided Morgan with numerous pieces of technical information bearing on their domestic operations during the war. (Still kept secret, interestingly enough, are such "vital" statistics as the nature of the securities held in the wartime "Currency Notes Redemption Account," a complete breakdown of "Government" and "Other" securities held by the Bank of England, and the amounts of wartime "Special Deposits" with the Bank.)

On the basis of these new data, plus judicious use of published statistics and earlier studies, Morgan makes many improved estimates. Such include: *internal* wartime government expenditures and revenue; the impact of the war on the

distribution of holdings of the national debt; a complete wartime balance of payments on current *and* capital account; and the wartime deterioration of Britain's net short and long-term creditor position. Morgan's work will be a valuable source book for close students of British financial history.

JAMES S. EARLEY

University of Wisconsin

Economic Forces in American History. By GEORGE SOULE. (New York: William Sloane Associates. 1952. Pp. viii, 568. $4.75.)

The structure of George Soule's economic history of the United States is unusual. The first 218 pages (Part I) relate to developments prior to 1900, while a larger portion of the text, the next 313 pages (Part II), covers merely the first half of the twentieth century. The emphasis on recent history is explained by the author as the result of an endeavor to illuminate the current scene, which exhibits a much greater complexity of the economic process than do the earlier years. The first part of the book begins with a brief chronology of events up to 1900. This is followed by a statement of seven themes for the period and an equal number of chapters—one dealing with each theme and each introduced by a short summary. In Part II the order of presentation is reversed: seven chapters dealing with particular topics are followed by a four-chapter chronological coverage of the post-1900 period.

To present a detailed discussion of the most important topics and at the same time to make the reader conscious of the broad aspects of development is the most difficult task of the historian. Each section of Soule's book, whether it is a discussion of one topic over time or a general chronology of events, provides a good description supplemented by an adequate amount of what might be called "short-run" analysis. The book is essentially a collection of very good essays, which are not effectively tied together—as far as this reader is concerned—despite the author's use of chronologies.

The failure to give the reader a feeling of economic development is a serious defect and has a two-fold basis. Part II is quite independent of Part I; it is as if the post-1900 events had no relation to those that preceded. Secondly, the author neglects to stress the broad underlying forces that account for general developments. His explanations of developments are in terms of short-run phenomena. A prosperous period, for example, is explained in terms of what has taken place in the immediate past (or takes place as prosperity grows) in agriculture, in certain specified manufacturing industries, etc. But, except for a chapter dealing with technology, no more than passing references are made to long-run forces of development. No stress is laid on the widening of the market for goods that occurred as a result of railroad expansion, and no connection is built between the widened market and a number of phenomena of the late 19th and early 20th centuries: the "trust" problem, the labor problem, various problems of urban growth, and so forth. Concerning the automobile, it is just as necessary, it seems to me, to write in terms of the wider market it has given labor and the numerous long-run repercussions of such a

widened market, as it is to write in terms of the changes in the demand for (and supply of) automobiles and the repercussions of these changes on a phase of a given business cycle. The latter Soule does well; the former gets no attention. Such omissions give the book a lack of unity that evolutionary threads would provide.

In many ways the presentation is very good. After the reader gets over the first few pages of "fine" phrases, the separate chapters will appear as excellent essays. This is particularly true of the chapters of Part II where the author treats technology and the volume of production, various aspects of national income, the rôle of money, international economics, the labor force, the ups and downs of the farmers, the relations between government and business, and the events of the last fifty years in a well-organized running account. Some readers may object to the favorable attitude toward the increasing rôle of government in the economy, but only ardent opponents of intervention will be offended. The short-run forces are very well handled, but the scant treatment of the long-run, basic, fundamental forces is disappointing in a book entitled *Economic Forces in American History*.

G. Heberton Evans, Jr.

The Johns Hopkins University

Report on Cuba. Report of a Mission of the International Bank for Reconstruction and Development. (Washington: International Bank for Reconstruction and Development. 1951. Pp. xxiv, 1,049. $7.50.)

This book, the work of sixteen authors who comprised an International Bank Special Mission under Francis A. Truslow, is a virtual encyclopedia on the economy of Cuba. There are chapters on topics ranging from Mineral Resources to A Bag Mill for Cuba. The first two chapters present the diagnosis and recommendations, while the remaining 48 chapters cover specific problem-areas that are relevant to an analysis of the development task in which Cuba is engaged. The book abounds with descriptive material of significant reference value.

As would be expected, sugar is the target of extensive critical analysis. The dominant theme is that too large a part of Cuba's total economic effort is in sugar and not enough in a diversified list of other products. Sugar is a seasonal industry, offering employment mainly over a period of four months beginning in the middle of winter. Moreover, the product is subject to the vagaries of price fluctuations traceable to Cuba's rôle as a residual supplier in the world market. By diversifying its output, Cuba could make more effective use of labor resources, reduce the impact of varying prices and export outlets for sugar, and perhaps moderate the disincentives of the unenterprising gambling spirit that is said to be a by-product of heavy reliance on the changing fortunes of sugar.

A variation on the main theme is labor policy, particularly the high wage demands and featherbedding that characterize contemporary Cuban labor. Such labor demands and practices, we are repeatedly told, constitute a stum-

bling block to industrial and agricultural diversification that is only second in importance to the national habit of pinning hopes on sugar. If there is anything to the idea that wage levels are established in export industries, however, it seems to me that the complaints of Cuban development enthusiasts and the Mission really demonstrate the strength of sugar's comparative advantage rather than many of the things mentioned in this *Report*.

Another point that recurs throughout the volume is the reluctance of Cubans to invest their savings in ways that accelerate the economic diversification of the economy. There is too much investment in real estate and American securities, and too great a propensity to hold idle balances. But are rates of return, the necessary allowances being made for special (and very great) risk factors, really more attractive in many of the lines that accord with the diversification thesis? Since there is little in the *Report* to indicate that the motivation of Cuban investors is significantly different from that of our own people, one wonders whether anything like the appropriate allowance for risk is made by those who are critical of Cuban investors. In fairness, however, it should be pointed out that the Mission calls attention to the need to improve the investment climate in Cuba. If this is done, and it can only be done by the Cubans, perhaps the resulting pattern of investment in due course will yield the much-desired returns in terms of a solidly based diversification of the economy. Domestic (gross) savings, incidentally, averaged about 13 per cent of Gross National Product during 1945-49 (p. 516)—a fairly respectable figure, even after allowance is made for the favorable position of sugar at the time. Although such an investment climate would do much to facilitate augmented foreign investment in the country, the *Report* is strangely silent about this aspect of the development problem.

Mention may also be made of several of the more important general recommendations of the *Report*. It calls for a re-examination of Cuban-American tariff preferences, urges that steps be taken (despite the restrictions of bilateralism) to expand the sale of sugar in Europe, recommends (with praiseworthy emphasis) against the establishment of a steel industry in the light of resource deficiencies, market size, and economic proximity to the United States, advises that policies should continue to stress the value of exchange-rate stability without exchange control, and recommends that monetary and fiscal measures which aim at general economic stability be restricted to those that are consistent with exchange-rate stability.

One misses a general statement of the theory of economic development, especially in its application to Cuba, a country that is among the most developed of the underdeveloped areas. If a chapter or two had been devoted to such a theoretical statement, readers would have been able to obtain a clearer idea of the Mission's conception of the development problem. We are all for economic progress in the form of rising per capita production, especially when it is not entirely or mainly at the expense of others than the nationals benefiting therefrom. In particular, such a theoretical statement would have been helpful by setting forth the chief assumptions involved. Moreover, I venture to assert that if a careful theoretical statement of about 5 per cent of the size

of the volume had been written, much of the descriptive material would have told a more persuasive story, the alternative opportunities for using labor during the "dead season" in sugar production would have been stated more effectively, the length of the *Report* considerably shortened and repetitive material omitted, and the necessarily slow process of development more clearly demonstrated. The last point in particular deserves emphasis. Strong emphasis on the process of development would have integrated the various parts of the *Report* and indicated in a more logical way the responsibilities of individuals, firms and government if given objectives are to be met. There is every reason to believe, however, that the volume should have more than a modest guidance value to the leaders of the Cuban community upon whom, in the last analysis, the task of development falls.

VIRGIL SALERA

Washington, D.C.

The Economic Development of Guatemala. Report of a Mission sponsored by the International Bank for Reconstruction and Development in collaboration with the Government of Guatemala. (Washington: International Bank for Reconstruction and Development. 1951. Pp. xviii, 305. $5.00.)

Public Finance and Economic Development in Guatemala. By JOHN H. ADLER, EUGENE R. SCHLESINGER AND ERNEST C. OLSON, in collaboration with the Research Department of the Banco de Guatemala. (Stanford: Stanford University Press. 1952. Pp. xix, 282. $5.00.)

An International Bank Mission, headed by Dr. George E. Britnell, has surveyed Guatemala's potentialities for development "in order to enable the Bank to make recommendations with respect to the general directions in which such development could be most fruitfully undertaken and the conditions required for its success." In the report, published by the Bank, the Mission states that its development program has been influenced by three criteria. It has endeavored to allocate resources, first, "into fields which promise early . . . improvements in income and the standard of living"; second, into fields which promise cumulative improvements in income and the standard of living; third, to accomplish these two "without imposing an excessive strain on the country's ability to maintain a sound balance of payments position."

Accordingly, top priority is given to the promotion of coffee, the main export crop, and to the mechanized development of the richest but underutilized food producing area. Coffee production offers the greatest possibilities for early returns and is also the "outstanding potential source of the increased revenue and foreign exchange needed to carry out a national development program." At the end of the tenth year of the proposed plan, the Mission expects coffee exports to be fifty per cent greater than the present level. The highway development plan, which accounts for a large percentage of total development expenditure, is also designed to facilitate the rapid expansion of valuable output which will produce foreign exchange quickly. The mechanized food pro-

duction program is expected to provide "a prompt and substantial increase in foodstuffs at reduced prices . . . [and] a better supply of raw materials for industry."

The first criterion which influenced the Mission's recommendations is an application of the marginal analysis and, by definition, will produce the greatest addition to real national income that could be obtained from given resources at any one time. The third criterion is used more as a note of caution limiting the scope of the development program than as a guiding principle for determining the desirable contents of that program. However, the marginal principle (first criterion) is not necessarily compatible with the growth (second) criterion. For example, if instead of being invested in conformity with the principle of short-run marginalism, more capital were invested in developing an economic structure which would yield a higher rate of growth than existed formerly, although the level of real national income would be lower in the short run, it would be higher in the long run.

As indicated, criteria one and three, based upon marginalism and prudence, are quite adequately satisfied in the plan; but how much emphasis has the Mission placed upon its second criterion? The growth criterion may best be satisfied if a "concatenation of industries" is developed. Such a "concatenated" structure has these attributes: first, an increase in the output of any one consumers' good or export, x, may increase the demand for and the output of other domestically produced goods which, in turn, react back and stimulate the demand for and the supply of x. The process then repeats itself. If the supplies of the various consumers' goods act as incentive goods to the other producers, or if the demand for the consumers' goods is complementary, such a concatenation may easily occur. Moreover, it should be possible for any major industry to serve as the initial stimulus for the expansion of the rest, in contrast with an export economy where the fates of the other forms of economic activity are completely dependent upon the vagaries of the export crops.

Secondly, the "concatenated" industries utilize a large volume of inputs which can be produced, or are available, at home. Whenever the output of consumers' goods is stimulated, in the manner just described, further production or fuller utilization of inputs is induced. Additional incomes are thereby generated and the relatively unskilled and underutilized labor force may obtain further training. This, in turn, enables real incomes to grow further since increased output of goods and exports may be stimulated either by higher incomes or by greater productivity of the inputs. Thus, the growth process is started over again. Such an economic structure does not necessarily involve any contempt for the principles underlying the gains from trade. Needless to say, this is not a short-run process, especially in backward areas; but is the process of growth ever a short-run affair?

Given this analysis, how far has the Britnell Mission's plan been influenced by its growth criterion? The Mission gives top priority to the development of coffee, corn and cotton production, but is noncommittal concerning the priorities of industrial growth. Thereby, the plan's ability to satisfy the

Mission's growth criterion is somewhat lessened. Food, exports, cotton and textiles are mildly "concatenated" to provide the foundations for economic growth in the sense discussed above. However, the Mission does not specify that the development of the textile, shoe and leather industries should receive priority over the development of the beverages industry. The choice as to which industrial products are to be developed is an important one. An economic structure based upon the coffee, food, cotton, textiles, shoe and leather and livestock industries may very well satisfy the growth criterion. On the other hand, an economic structure based on coffee, food and beverages may be unable to satisfy this criterion. The fortunes of coffee will affect those of the latter two, but it is difficult to see either how an increased demand for food or beverages might be induced any other way, or how an increased demand for one of the latter two could stimulate substantial growth in the others. What domestic inputs would be stimulated, and would such a structure provide training for labor?

The heavy orientation of this medium-run program towards criteria one and three may not be unjustified since it may be necessary to increase the relative emphasis upon projects which mainly satisfy these criteria, if the most desirable projects according to the growth criterion are unable to cope adequately with the foreign exchange and inflationary dangers. A long-range development program, however, should definitely place major emphasis upon the growth criterion, since the main value of a development program is its ability to create an economic structure which will generate its own rate of growth at a higher level than existed formerly.

This interesting report will probably be read by many people who will be called upon to implement development programs in Guatemala and in other countries. Such people, particularly, would profit more from the Mission's report if it had developed more fully the factors which influenced its decision to place major emphasis upon criteria one and three instead of upon the growth criterion.

The authors of the second volume under review have undertaken a type of study which is essential to a rational formulation of any program for economic development. Their study examines the impact of the Guatemalan fiscal operations upon the economy, the effect of these operations on the rate of economic development, and the extent to which the fiscal system can contribute to economic development. Since an important way in which fiscal policy may influence the rate of economic growth is through its effects upon the profitability—both before and after taxes—of different industries, the authors devote some attention to the impact of the fiscal system on business. Here, they analyze the effects of taxes upon resource allocation by determining the different tax burdens borne by various industries and the effect of the tax structure upon the form of business organization, the size of business firms and the structure of production.

An extremely suggestive and interesting observation is made concerning an indirect relation between the tax system and the structure of production. Since the market structure is very conducive to tax shifting—about eighty per cent

of the total business taxes are shifted to consumers—it is concluded that taxes weigh most heavily upon products having high income elasticities of demand. From this, one may infer that inferior goods consumed by the nonbusiness sector, which bears the incidence of these taxes, and luxury goods, consumed by businessmen (in the broad sense of the term) who shift these taxes, would gain from taxes which could be shifted, while taxed products which have very low price elasticities of demand would not suffer very much. Unfortunately, the available information precludes any more definite statement as to which products would be affected by these indirect effects. However, if the products which are hurt by these taxes are those whose development the government is trying to stimulate, then the use of such taxes may be inimical to the government's economic development program. On the other hand, in Guatemala taxes which can be easily shifted curtail consumption and thus may be effective anti-inflationary devices. Therefore, it is to be hoped that the authors may in a future study evaluate the desirability for economic development of a tax structure which is based heavily upon taxes which can be easily shifted.

Fiscal policy can also influence the rate of economic growth by combating inflation, which often is induced by a development program, and by mitigating the impact of cyclical fluctuations. The authors, therefore, raise three basic questions to be answered in the latter part of the book which is mainly concerned with aggregates. First, is the revenue system "adequately flexible for a process of gradual expansion of general economic activity *without* inflationary price increases?" Second, is the present fiscal system capable of stemming "an inflationary tide" which may result from development? Third, what is the "proper degree of revenue flexibility" if the impact of cyclical fluctuations is to be mitigated?

Since a large part of the revenue depends upon the volume of imports which, the authors believe, increases more than proportionately with real income, it is stated that the fiscal system is sufficiently flexible to finance the gradual growth of output, if inflation is avoided. It is, therefore, maintained that a strong case can be made for continuing to rely heavily upon the yield from import taxes as a major source of government revenue. It is to be feared, however, that this policy recommendation may be misunderstood by the fiscal authorities. The case for a continued heavy reliance upon import taxes must presuppose that exports will grow proportionately with imports. Such a presupposition is not without justification because a large growth of exports is planned by the Britnell Mission in the near future. But, how long is this growth of exports likely to continue? If imports tend to rise more than proportionately with real income and if exports do not, will not the resulting balance of payments difficulties force the volume of imports to conform to the available foreign exchange reserves? Import tax revenues will not be able to continue to grow at the present rate since the growth of imports will be restrained by government decree. Furthermore, it is probable that a long-range development program will reduce the relative growth of exports to real income.

Therefore, since the long-run growth of exports cannot be assumed to equal the growth of imports—before government intervention—the fiscal authorities should be cautioned that they may be forced to modify the tax structure,

gradually, to rely less heavily upon import duties for a major part of government revenues.

The authors' analysis of the "proper degree of revenue flexibility" to mitigate the impact of cyclical fluctuations, and of the revisions in the tax structure which are designed to correct an inflationary problem, are further examples of how their policy recommendations and analyses give thoughtful consideration to the unique structure of underdeveloped countries and avoid the facile application of the usual economic rules of thumb which have emerged from the experiences of industrial countries.

JEROME L. STEIN

Wesleyan University

Evolución Histórica de la Industria Siderurgica Chilena e Ibero-Americana. By CARLOS SANCHEZ HURTADO. (Santiago, Chile: Editorial Nascimento. 1952. Pp. 398.)

There are few Latin Americans today who do not want to industrialize their countries. Those who oppose the establishment of heavy industry—specifically an iron and steel industry—are almost as uncommon. There are now iron and steel industries, or more or less well-advanced plans for bringing them into existence, in Brazil, Chile, Mexico, Argentina, Colombia, Peru, and Venezuela. Even the smaller nations are thinking in terms of establishing metal fabricating industries. The iron and steel industry has come to represent economic maturity in the southern republics of the hemisphere.

The present volume deals with this problem in two parts. The first is a study of the development of the iron and steel industry in eight countries in South and Middle America other than Chile; the second and larger half concerns the development of that industry in the shoestring republic. The author, a Chilean, is particularly interested in the development in his own nation, and is concerned with the industry in the other countries principally in so far as it throws light on the situation in his own.

Sanchez Hurtado is an enthusiastic supporter of the new Huachipato steel plant which first entered into production in Chile in 1950. He examines with considerable care the problems which have faced this enterprise, and how they are being overcome. For instance, he looks at the prospective markets for the plant's products, concluding that a large enough market is likely to exist in Chile for a plant of the present capacity of Huachipato, and hints that in time there may even be sufficient markets in Chile and the neighboring countries for a plant capable of producing at the most efficient capacity, according to United States standards, which would mean a plant about three times the present size of Huachipato. His analysis of this possible expansion of plant rests fairly heavily on the likelihood of an export market for a considerable amount of output in Argentina, which will probably never have an iron and steel industry capable of meeting its own needs. It rests less heavily on the possibilities of exporting to the neighboring countries of the Pacific coast of South America.

The author examines the raw material situation of the industry, analyzing

Chile's resources of iron, coal, electricity and other required products. Although there is plenty of iron, and of most of the others, the possible Achilles heel is coal.

Problems of location, and difficulties in obtaining a labor force and technical skill, are also discussed. Finally, Sanchez Hurtado describes in some detail the financial arrangements which were made with the Export-Import Bank. He stoutly defends the terms of the agreement with the Eximbank, terms which because of their requirement that American engineers and technicians administer the plant in its five years, have come in for a great deal of attack in Chile.

This study is a product of the Seminar in Economics of the University of Chile. This seminar, directed by some of the country's leading economists, has been responsible for some of the best studies of Latin American economic life which have been made. The present work takes its place as one of the most distinguished of these research projects. It is scholarly in its research and scope. It has an exceedingly useful bibliography appended.

Robert J. Alexander

Rutgers University

The Economic Development of Iraq. Report of a Mission organized by the International Bank for Reconstruction and Development at the request of the Government of Iraq. (Baltimore: Johns Hopkins Press. 1952. Pp. xi, 463. $5.00.)

This report might have been sub-titled: The Art of Spending Large Funds Wisely, or, Inflation in Moderation. The International Bank's mission to Iraq, headed by Ivar Rooth, with John C. de Wilde as chief economist, had the unique and enviable task of making recommendations for an economic development program in a situation in which the problem is not a cramping shortage of funds but, rather, the effective use of a virtual flood of revenues for development.

In accordance with an agreement reached in 1951, the three internationally owned oil companies operating in Iraq have undertaken to raise oil output, which amounted to 6 million tons in 1950, to some 30 million tons per year by the end of 1955 and to turn over to the Iraqi government half of the profits before the deduction of taxes. On this basis it is anticipated that over a five-year period the government may receive net revenues amounting to as much as 214 million dinars from oil alone. (One Iraqi dinar equals £ 1 or $2.80.) Under the terms of a law passed in 1950 all such oil revenues are to be used for developmental purposes.

To carry out the proposed five-year program, the government will have "to gear itself" to an annual rate of development expenditures which at the end of the period will need to be about ten times the total public capital outlays (ID 5,200,000) in 1950-51. The mission's report stresses the dangers involved and certain of the basic requirements. "The experience of other countries in process of development illustrates that it is not easy to spend money rapidly

and efficiently on well-conceived projects. . . . Such a rate of expenditure will be achieved only with great effort and by careful planning."

The mission's major prescription for preventing a severe and disruptive wage and price inflation is for the government to allow liberal use of its sterling income from oil to pay for imports. Measures to curb credit expansion are also proposed. Lacking reliable information at almost every point, the mission must skirt many of the critical issues: Granted the wasteful use of manpower on the land, can a sufficient labor force be recruited rapidly, and can it be done without a serious drop in agricultural production? Certain types of skilled labor are already in short supply; will the development projects tend to siphon off this supply and disrupt the existing industries and services? In the face of the obvious difficulties posed by the unprecedented situation and the lack of information, the mission provides a thoughtfully conceived set of general guide-lines, including a recommendation that a committee of economic experts be formed to keep watch on the nature of the changes that the economy is undergoing to provide a basis for more detailed planning.

The report provides a brief, but careful, analysis of the underlying economic problems of the country and a detailed description of the major problems and potentialities within the agricultural and industrial sectors. Iraq has a relatively rich natural resource base. In addition to its fabulous oil resources, it has a vast land frontier. Official estimates suggest that the amount of "cultivable" land is almost three times that under cultivation. Projects for the storage of the flood waters of the Tigris and the Euphrates can open up large new areas for settlement, while the drainage of lands which have deteriorated owing to salting could do much to raise the present low productivity of agriculture.

Industry is as yet little developed, but here too the mission finds conditions favorable to fairly rapid expansion. In oil and natural gas the country possesses a cheap source of power and fuel as well as an important source of raw materials. An expanding agriculture will provide a variety of materials for processing. The mission makes specific suggestions as to what industries might be expanded and what new industries might be established, based on an analysis of the prospect for efficient production on a competitive basis.

The existing transport system is found to serve the present needs of the country adequately and, with the implementation of programs for improvement of the road network and the railway system, transport capacity should have no difficulty in meeting the growing requirements of agriculture and industry.

The great hurdles to rapid economic progress are seen to be not in the realm of natural resources and financing, but in the "underdeveloped" state of the human resources and in the generally backward-looking and often exploitive institutional arrangements which dominate the Iraqi scene. The Iraqi *fellah* lives in the direst poverty and suffers from debilitating diseases; he is illiterate, ignorant and tradition-bound. Also, he is exploited (he sometimes has to turn over as much as six-sevenths of his crop to the landlord) and he is overtaxed.

Throughout the report, the mission stresses the importance of improvements in the quality of the human resource. Several chapters are devoted to measures for raising the levels of health and education and expanding the basic community services. The most advanced thinking in these fields is drawn upon and the whole subject is treated with thoroughness and insight. An unusual feature of the report is a section on community planning and the rôle it can play in improving the living conditions of the population and the efficiency of the public services.

Far less satisfactory is the treatment afforded the hurdle to progress posed by existing institutions. The report refers to a few of the difficulties—for example, the tendency for new and improved land to fall almost entirely into the hands of the big landowners, and the excessive, monopoly-creating protection of existing industries—but the few minor changes proposed would seem to fall far short of providing for a "climate" in which the ambitious targets set could possibly be achieved. Here, of course, is involved the difficult question of how far a "foreign" mission can and should go in discussing underlying social, political and economic institutions. Under any circumstances, one might question the advisability of setting targets and proposing programs involving unprecedented rates of progress, especially for a country which for many centuries has remained virtually static, without at the same time specifying fully the conditions for such progress. Expert missions have the opportunity—and, it may be argued, the responsibility—of treating problems within their usually narrow terms of reference in a context which relates economic development to broad social issues and provides a challenge to aim at progress on a truly broad front. Within its own terms of reference, the mission to Iraq has provided a competent and valuable report.

Harvey S. Perloff

University of Chicago

Challenge and Response in the Middle East. By Hedley V. Cooke. (New York: Harper & Brothers. 1952. Pp. xiii, 366. $4.00.)

Recent events in the strategic Middle East should not baffle those who have read Hedley V. Cooke's basic and timely study. The book explains in some detail the circumstances which underlie the unrest and revolution in Egypt, the thorny problems which stand in the way of a rational settlement of the Iranian oil question, the rapid strides which Israel and Turkey have made since the end of World War II.

Challenge and Response in the Middle East is an account of the economic, political, and social conditions of the area, showing how these conditions arose, are perpetuated, and what can be done to improve them. The book opens with an introduction and follows with chapters on each country of the region—Iran, Iraq, Syria, Jordan, Israel, Egypt, Turkey, Lebanon, Saudi Arabia, and Yemen. A chapter on regional planning and one on the future conclude the study. The country chapters provide, for each political entity of the area, factual and analytical material on the general background; the nature,

application, and the results of the various plans which have been made for economic and social improvement; and recommendations for future action.

This study should appeal to a wide audience, and find a place on the reserve reading shelves of college courses in economics and geography. Both well-written and scholarly, it could be read with profit by those interested in this strategic region and America's rôle in international economic development. In particular, economic planners, the institutionalists, regional and agricultural economists, geographers, as well as development engineers, will be amply rewarded by a study of this document.

In part, this work is the story of what exaggerated emphasis upon nationalism, xenophobia, tradition, together with unenlightened class interest, can mean in terms of physical impoverishment. It is easy to see how the Middle East pattern of income distribution has retarded the rate of economic development, with its great inequality between very narrow upper and very wide lower limits, and with its heavy emphasis upon incomes from property as against those from personal effort.

Hedley Cooke offers no pat solution for the problems of the area. Although he apparently favors the regional approach, he is cognizant of its pitfalls. He seems to have considerable confidence in the capacity of Point IV to aid in the development of the area—but one wonders, in view of the magnitude of the task, if sufficient funds are available to do more than scratch the surface of the problem.

Two possible avenues to the development of the area—private investment and trade promotion techniques—are not explored in any detail, although the investment experience of the oil companies in the Middle East and the results achieved by the American and European trade promotion programs afford good case material on both methods.

The study sketches, somewhat too briefly perhaps, the work done by the Middle East Supply Center, an Anglo-American organization created during World War II to assure a minimum flow of essential goods to the region and to maintain its political stability. This organization, which had the confidence of virtually all groups of the area, was liquidated at the end of hostilities together with Lend-Lease. In view of the substantial achievements of the Middle East Supply Center during World War II, its continuation, together with Lend-Lease, into the post-war period might have proved of considerable aid in the development of the Middle East.

Economic planners will find little solace, but much food for thought, in the author's analysis. The Middle East has not suffered from any dearth of planning. The plans, by and large, have not been carried out and this work spells out in some detail, the reasons for these failures. Institutionalists, on the other hand, will find much evidence in the pages of this book, of the power of the institutions—especially those of land tenure—to influence the level and the pattern of economic life.

Hedley Cooke assembles and analyzes the results of recent scholarly investigations and adds material and insight gleaned from several years residence in the region as a Foreign Service officer. The result is a top-notch contri-

bution to our understanding of the problems of the Middle East, readably presented with objectivity and competence.

MAX J. WASSERMAN

Howard University

The Soviet Financial System: Its Development and Relations with the Western World. By MIKHAIL V. CONDOIDE. (Columbus: Bureau of Business Research, College of Commerce and Administration, The Ohio State University. 1951. Pp. xiii, 230. $4.00.)

This book is notable for the fact that it represents the first attempt by a Western student in almost fifteen years to undertake anything like a systematic survey of the Soviet financial system. It seems curious that this aspect of Soviet economic activity should have received so little attention in the West, particularly since this is one field in which the Soviet government's restrictive information policy has been applied with relative leniency. A considerable volume of descriptive as well as statistical material on Soviet finance continues to appear from Moscow. While this material is notoriously fragmentary and often difficult to interpret, it does present the meticulous student with a promising opportunity to examine and appraise the functions of Soviet finance and its rôle in Soviet economic development.

Professor Condoide's study, unfortunately, does not take full advantage of this opportunity. Its positive contribution is a description of the legal-institutional framework of Soviet finance. On this level, it examines the organizational structure and operational details of the banking and credit system, defines the legal status of money and gold, describes the revenue and expenditure composition of the government budget and outlines the methods of financing foreign trade. The material is illustrated with a fair collection of summary statistics and supplemented by a detailed bibliography and some interesting documentary appendix material. However, the usefulness of this descriptive material is impaired by some notable omissions (*e.g.*, no mention of the State Insurance System, no data showing outlays on "internal security"), by factual inaccuracies (*e.g.*, faulty statements on savings bank interest rates and services, p. 45; wartime changes in tax rates, p. 96), by errors of interpretation (*e.g.*, confusion between national budget and financial plan, pp. 77 and 160), and by some irrelevancies (*e.g.*, 40 pages of text and appendix devoted to Soviet foreign policies and violations of treaty obligations).

In its analytic aspects the study is disappointing. In his attempts to analyze the functional aspects of Soviet finance and to assign finance its proper rôle in Soviet economic organization, Condoide is hampered by a superficial understanding of Soviet economic processes. His statements that "the allocation of labor is authoritative" (p. 13), that "all economic activity is financed by the state" (p. 20) and that "the state is the sole producer and distributor of commodities" (p. 20) depict the central authority as a ubiquitous and omnipotent decision-making entity and completely ignore the important rôle that decentralized decisions and nongovernmental organizations play in Soviet

economic activity. His comments on the functions of money, credit and prices are often vague and sometimes contradictory. Thus he claims, on the one hand, that "the government's investment plan is carried out automatically, as a result of the allocation of physical resources, no matter how it is financed" (p. 14), and concludes, on the other hand, that "Soviet banking, credit and monetary policies provided practically unlimited funds to promote industrialization; hence what provided employment was this spending and not planning as sometimes assumed" (p. 47).

In his analysis of financial policy the author shows a tendency simply to reproduce, without further comment, the ambiguous and cryptic official Soviet stereotypes which purport to explain the objectives and rationale of their financial operations. His efforts to probe behind these stereotypes show little insight. Thus, while he takes issue with the Soviet claim that gold reserves are related to the tempo of socialist development, he assumes that the pegging of the ruble to gold will make gold reserves "to a much greater extent than heretofore, the foundation and security of the Soviet currency system" (p. 58). Condoide dwells at length on past Soviet failure to hold in check the inflationary forces generated by Soviet industrialization, but neglects to point out that, at least since 1948, the hard lessons of monetary instability seem to have been learned. In this connection, a discussion of the use of the budget surplus as a fiscal counterpart to bank credit creation, and other deflationary techniques applied by the Russians with conspicuous success in recent years, would have been welcomed.

To the general reader, this book may prove helpful as an introduction to the financial institutions of the USSR; because of its analytic deficiencies, however, it cannot be recommended to the specialist.

HANS HEYMANN, JR.

The RAND Corporation
Santa Monica, California

Labor in the Soviet Union. By SOLOMON M. SCHWARZ. (New York: Frederick A. Praeger. 1952. Pp. xviii, 364. $6.00.)

Dr. Schwarz notes at the outset of his book that he is considering only certain topics within the larger field of subject matter which the title connotes. His concern is with the theory and practice of Soviet labor policy in four areas: the labor market, wages and the standard of living, hours and conditions of work, and social insurance. Furthermore, he deals only with that portion of the total labor force which works for hire, the "workers and employees." They comprise the overwhelming bulk of the industrial (and urban) labor force, but they are less than half of the total.

If there is a theme in his book, it is that Soviet labor in the four areas is far from as well off as it could be were official policy otherwise and as the Soviet press would have the people at home and abroad believe it is. This means something different in each of the areas, and the book can therefore best be reviewed one area at a time.

Compulsory control is the aspect of Soviet policy in the labor market which receives most of the author's attention. The discussion centers on the labor laws of 1938 and 1940. In certain of their provisions, designed to strengthen plant discipline and curb labor turnover through "moderate" compulsion involving pressures and penalties, these laws are an extension of past laws. Schwarz describes the development of this type of legislation in great detail, giving special attention to its origin in the transition from the New Economic Policy to the First Five-Year Plan. In their more drastic provisions, however, the laws of 1938-40 represent "a fundamental change in the labor relationship" (p. 100): the introduction of workbooks, the establishment of a labor draft, provision for the compulsory transfer of certain skilled personnel, and the ordering of criminal punishment for quitting a job without permission of the employer. Even here, the author finds some precedence in earlier legislation.

These laws gave to individual ministries and enterprises compulsory control over the labor in their jurisdiction. Whether the absence of a central administration of labor control (except for the training program) made this compulsion any the less pervasive from the standpoint of the worker, the author does not clarify: ". . . practically all elements of . . . compulsory labor were at hand . . . lacking [only] an administrative setup to make it operate as a coordinated compulsory organization of work processes" (p. 119).

On the question of postwar developments in the labor market, Schwarz is very brief. After noting that extraordinary measures adopted during the war were apparently dropped after 1947, he says only this: "The enforcement of the decree of . . . 1940 seemed . . . uncertain in 1945 and 1946. This . . . was . . . a passing phase. The government . . . strove . . . to overcome the weakening of their rigid system . . . [and] in the field of labor policy . . . seemed ready for a consistent pursuit of the course . . . strikingly expressed in the 'reform' of June, 1940" (pp. 128-29).

The chapters on wages and the standard of living cover the movement of money wages and consumer goods prices, in order to show what has happened to real wages since 1928. The presentation is detailed, but the author does not attempt, as others have, to construct an index from the incomplete price data. Consequently, his conclusions on the movement of real wages are in general terms: Real wages dropped about 50 per cent from 1928 to 1932, rose about 20 per cent from 1935 to mid-1938, and then fell continuously until 1946. A rise after 1946 is still going on; but he is sure that real wages in 1928, 1938 and probably even 1940 are "far from attained" at present. He summarizes the views of other analysts on this question and takes issue on certain points. Within an area of general agreement on developments since 1928, his is the relatively pessimistic view of recent wage trends.

The author estimates that "socialized wages" (*e.g.*, public health) show a trend which more or less parallels the movement of real wages; therefore, "it can hardly be concluded that they . . . make up for the latter's decline" (p. 249).

Three-quarters of the book is devoted to the sections above on the labor market and wages and living standards. The treatment of the third subject,

hours and working conditions, includes the time-organization of production, from the seven-hour three-shift day with continuous work week used early in the plans to the present eight-hour day and 48-hour week; the practice of "socialist competition" (up to 1937); the widespread use of overtime and the violation of restrictions on the use of female labor (called a "decline in labor protection"); and the deterioration in work safety. On postwar developments the author gives only a summary impression, namely, that hours and the work-day are the same as in 1940, although overtime is probably not as prevalent, and that with respect to the decline in labor protection and safety, "a certain leveling off . . . seems to be in the making" (p. 303).

The final section of the book, on social insurance, is a short but comprehensive survey of legislation in the field. The theme is that social insurance, more than providing security for the worker, is a weapon in the drive to raise productivity and strengthen labor discipline.

This book evaluates Soviet labor policy in terms of its effect on the Soviet worker, and it finds that the cost of the policy in freedom and material well-being has been very high. As a result, many of the goals of the revolution and the five-year plans are seen betrayed. Other books on Soviet labor have reached the same conclusions,[1] but Schwarzs' treatment of the theme is noteworthy because he develops it more systematically, in greater detail, and with more documentation—and because he is able to include wartime developments. (His treatment of the postwar period is a limited one, as noted.) Furthermore, the book has unique value in the person of the author, a lifetime student of Russian and Soviet labor, whose commentary on specific points and issues will be of lasting benefit to specialist and nonspecialist alike.

In the end, on the grounds of both universal ethics and the lofty aims of the Bolshevik revolution, Soviet labor policy must be judged as Schwarz has judged it, in terms of "its impact on the every-day lives of workers and employees" (p. vi), all other conditions aside. In the meantime, however, it must also be evaluated (if not judged) in terms of the chiefly impersonal ends which it was designed (also) to achieve—the increase of production and labor productivity, the lowering of costs of production, the effective distribution of the labor force, etc., in other words, the solution of the basic economic problems.

Schwarz does not address himself to answering these questions, but he does touch on them frequently, a fact which may lead the casual reader to associate his secondary observations with the extensive foundation built to support his main theme. This may not always be justified. For example, while examining the beginnings of compulsory control, he makes this observation concerning the labor market of the 'thirties, following the abolition of unemployment insurance (1930): "the market automatism of labor supply and demand vanished" (p. 51). To accept such an unqualified statement, the reader

[1] The two most recent are M. Gordon, *Workers Before and After Lenin* (New York: Dutton, 1941) and L. E. Hubbard, *Soviet Labour and Industry* (London: Macmillan, 1942). The first puts the conclusions in even stronger terms than Schwarz does. The second, while reaching essentially the same conclusions, places them in perspective alongside other aspects of the total picture.

must be given as thorough a presentation of all the aspects of the labor market as he is of only one of them. Compulsory control over labor distribution and discipline may have been the dominant (if not exclusive) factor in the "market" after 1940, but it does not seem to have been such in the middle 'thirties (according even to evidence elsewhere in this book). On the contrary, enough of the characteristics of "demand and supply," as economists understand the term, were still evident prior to 1938-40, that the term may be used as an apt generalization of much of the market's activity.

WARREN W. EASON

The Johns Hopkins University

Economic Systems; Planning and Reform; Cooperation

The Cost of Industrial Movement. By W. F. LUTTRELL, assisted D. A. COCHLIN, F. G. DAVIDSON, and J. F. THOMPSON. Occasional paper No. XIV of the National Institute of Economic and Social Research. (New York: Cambridge University Press. 1952. Pp. ix, 104. $3.75.)

In the British Coalition Government's famous White Paper on Employment Policy for a "High and Stable" Employment, and in the policy of all subsequent governments, regardless of party, the problem of industrial location has loomed large. This was natural since in certain "depressed" areas, of which the biggest were South Wales and the North East Coast, unemployment in 1933-37 was at least double the average for the whole country, and there was, besides, a large reserve of unoccupied women of working age. In the Distribution of Industry Act, 1945, also fathered by the Coalition Government, development areas were formed out of these depressed areas and manufacturers virtually forced to build any new factories there, if anywhere. This is perhaps the nearest approach Britain has made to positive planning, and it was rendered possible by the postwar shortage of building materials. If manufacturers in "congested" areas proposed to build extensions to their main factory rather than branches in the development areas, they might not get the necessary allocation of timber and steel.

Free-enterprise enthusiasts will naturally want to know the extent of the resulting damage to the economy; and in a most laudable attempt to assess the economic costs, if any, of their own policy of insisting on branch factories rather than extensions, the Board of Trade, the responsible government department, financed Birmingham University, University College, London, and the National Institute of Economic and Social Research, to enquire into the relative costs of the different locations. This volume is a preliminary "interim" contribution, but the general plan of enquiry is explained in Sir Henry Clay's preface.

Mr. Luttrell has devoted himself to comparing new branch factories, mainly in the shoe industry, started in places unused to the industry, though not

necessarily in a development area. He finds that in the first years of their establishment new branch factories have, on average, much higher total costs, but that they gradually manage to reduce costs so that by the third year the four branches studied that long had 30%, 19%, 13% and 4% higher total costs than their main factory. These different percentages were associated with differences in the training of new workers and in the provision of a nucleus of skilled workers. Distance of branches from main factories was also important because of accessibility to skilled workers in the old district and to management services of the main factory.

An unexpected discovery of these studies is the lack of knowledge of firms about the precise burden of their costs. Once the goodwill of firms had been obtained, with full access to their accounts, definite and fairly exact conclusions might have been thought possible. In fact, firms themselves seem in the dark or, if they appear to themselves in the light, it is often a false light. Mr. Luttrell comments, for instance, on the undue disregard of overhead in comparison to direct labour cost, though overhead costs may be two or three times as great. Whether firms know their own costs or not, however, is in the present British situation not so important as might be supposed. With full employment, unfilled vacancies and lag in filling housing needs it is often not a matter of getting labour cheaper or operating cheaper generally in one place than another, but of getting labour at all. Many a Birmingham firm that cursed the government for insisting that it build a branch in a development area rather than extending its main factory must now be thanking its stars (but not of course the government) for this bit of State planning. For with the present prosperity and over-employment in Birmingham, labour is not to be had, though still available in areas some distance away.

Mr. Luttrell is to be congratulated on presenting his case studies clearly and yet in detail. After a brief introduction (Chap. 1), he is careful to explain the scope and methods of his enquiry (Chap. 2 and 3) before describing (Chap. 4-5) his four detailed cases, three in the shoe and one in the engineering industry. But he is not just descriptive. In his final chapter he puts forward some tentative conclusions as to the importance of training labour and the difference between a branch thirty miles and a branch sixty miles from the main works—conclusions which cannot be said to be novel or to arise very clearly from the cost comparisons. But in this pilot enquiry it is good that the author is careful to contrast the straight comparison between branch and main works with the more relevant comparison between branch and an equivalent *extension* of operations at the main works. If main works is not fully utilized, extension is, by spreading overheads, likely to be cheaper. But this assumes that labour can be secured at the main works location. Under present British conditions of full employment this assumption is not realistic and Mr. Luttrell is right in soft-pedalling the comparison of branch with extensions to main plant, though it is the more logical comparison. We look forward eagerly to further publications of this government-sponsored research.

P. Sargant Florence

University of Birmingham

Marx Against the Peasant: A Study in Social Dogmatism. By DAVID MITRANY. (Chapel Hill: University of North Carolina Press. 1951. Pp. xiii, 301. $4.50.)

Populism in Eastern Europe has its roots in the deep discontent of the peasant class. The land hungry (often landless) subsistence farmer was primarily interested in the redistribution of land. Populist writers borrowed from the physiocrats all the arguments in favor of an "independent" peasantry, protected by a benevolent government against feudal and urban exploitation. Mitrany's book is a good example of Populist writing. As a disciple of Proudhon, he felt impelled to attack what he thinks are twin enemies of the independent peasant: the liberal and the Marxist economist. Both "regarded the agrarian problem from the angle of production rather than from that of social organization," both looked upon "large-scale production . . . as the first condition for general well-being." Capitalist farming and collectivization lead to the same end—the destruction of the small family holding, which, after all, should not be looked upon "merely as a means of living but as a way of life."

Large-scale farming (according to Mitrany), as exemplified by the English inclosures and the latifundia in Italy and Eastern Europe, did not raise the productivity of the soil nor emerge as "a better system through free competition" (p. 11). This is true, of course, of latifundia, a feudal anarchronism, which kept the peasants in bondage (hence overcrowding, malnutrition, and low productivity). The inclosure movement, on the other hand, complemented the industrialization of England by providing a surplus of agricultural produce to feed rapidly expanding towns, which at the same time provided new avenues of employment for those not wanted on the land. If, however, the surplus farmer had been permitted to stay on the land, the result would have been as disastrous as in prewar Poland, for example, where minute subdivision of the land reduced peasant holdings to agricultural slums.

There is a lot of nonsense in Mitrany's book about "peasant mystique," about "the pernicious effects which followed from superimposing Western capitalism upon a simple peasant society," and about rural overpopulation—the worst type of concealed unemployment—which, perverting the concept of full employment, he assumes to be a contributory factor to full employment! "A country eminently agricultural . . . is a country eminently poor and socially and economically backward." This basic fact, expressed by a Populist writer whom he quotes (p. 28), leaves Mitrany unconvinced. But farming in industrialized Western Europe—depending, of course, upon availability of arable land—succeeded in balancing cash crop and diversified production, which neither the latifundia type of large-scale farming nor the small subsistence farmer in Eastern Europe was able to achieve. Still more important is the fact, that the peasant class in Western Europe retained its social organization, and became solidified as the core of a traditionally conservative society which, unlike the semifeudal East, was not averse to rapid industrialization and absorption of the landless in urban centers. This is what Stolypin (Russian Prime Minister, 1906-1911) proposed to do in his great land reform in Russia. He had in mind the creation of a loyal well-to-do peasantry as a

balance against the city proletariat, an ill-fated attempt to stave off revolution!

Peasant-dominated governments inspired by Populist ideals had their chance in power after the First World War, and muffed it. Hasty land reforms gave "millions of poor sharecroppers and landless laborers . . . a new status as formally [!] independent peasants even if economically many were not better off than dwarf owners" (p. 94). The division of land resulted in a "reduced supply of food to towns" (p. 95). Mitrany chides Western economists who "simply looked upon the fall in the production and export of cereals as proof of a decline in productivity" (p. 100). Why, farming had changed now from farming for the market to farming for subsistence! The peasants, "in an economically stronger position, satisfied first their own needs before they started producing for the market" (p. 101). In fact, a change from backward latifundia into backward small holdings brought about a deterioration in the balance of payment position and a further decline in the standard of living in these countries.

Thus redistribution of land—creating innumerable dwarf holdings—actually retarded the economic development of these countries. Poverty and unrest, as before the war, became a permanent feature, especially after the disastrous price drop of farm products in the early 1930's. Military *coups d'état* and court intrigues finally succeeded in eliminating reform governments under peasant leadership. Yet Mitrany leaves the impression that a "victorious" Populism had emerged as a sort of peasant Third Force (see Ch. 10, "Not Capitalism, Not Socialism"). On the contrary, its ideology has now been discarded as no longer useful. Peasant leaders, now in exile, have accepted the Western type of agricultural organization (*i.e.*, capitalist production with its legal guarantees of property holding, profit making, noninterference, etc.) as their ultimate goal. This they pledged in the so-called "Williamsburgh Declaration of 1952," a platform for restoration of Western-oriented regimes. On the other hand, former Populists who remained and joined the various front governments seem to have consented (or been forced to consent) to the destruction of small-scale farming and to rapid collectivization on a Russian model as a *fait accompli.*

FREDERICK SETHUR

The City College of New York

Money and Banking; Short-Term Credit; Consumer Finance

The Growth of a Central Bank. By L. F. GIBLIN. (Melbourne: The Melbourne University Press. 1951. New York: Cambridge University Press. 1952. Pp. xi, 363. $5.00.)

Soon after extensive Australian financial legislation was passed in 1945, the Commonwealth Bank of Australia commissioned Professor Giblin to write a history of that bank covering a period as far back as the previous major

financial legislation, the Commonwealth Bank Act of 1924. The Bank could not have chosen an author with more intimate knowledge of the subject: Giblin was one of the first professional economists to have been employed by the Commonwealth government; from 1935 to 1942, he was a member of the Commonwealth Bank Board, and from 1938 to 1946, he was chairman of the Conmmonwealth Advisory Committee on Financial and Economic Policy. The resulting volume can be considered a kind of personal memorial to Professor Giblin; he died as it was in press.

This history, even though "official" and commissioned does not suffer from an excess of reticence. Giblin makes wholly clear the shortcomings of both men and laws in the struggles of the Commonwealth Bank to achieve central banking stature. He reveals the limited vision of the Bank's founders, the inadequacies of the laws under which it operated, and finally the limited objectives of those who managed the Bank during its early years.

One of the greatest mistakes was the effort to use the English model without sufficient adaption to local problems. The Commonwealth government patterned the Commonwealth Bank after the Bank of England. It tried to endow the Bank with similar powers and similar responsibilities. But the Commonwealth Bank could not discharge these responsibilities with these powers primarily because the pervasive influence of banking tradition did not operate as it did in the City of London. The Australian banks were able to challenge the financial influence of the Commonwealth Bank with impunity. Only the financial necessities of war finally forced the government to give the Bank real central banking powers. Perhaps the chief of these new powers was an enforced rule concerning the character and amount of commerical banking reserves.

Professor Giblin explains at length two banking developments not adequately reported elsewhere. During the war, Australian banks were not subject to excess profits taxation; as a result, one of the war duties of the Commonwealth Bank was to control the profits of banks. This was done by adjustment of the special accounts which will be mentioned in the next few sentences. The second feature of general interest was the introduction of secondary reserve requirements, somewhat similar to those the Federal Reserve sponsored in the early postwar years. This plan was the outgrowth of a system of war finance by which bank participation was managed by direct allocation to special accounts and not by voluntary subscription as in the United States. The obligations were similar to the treasury deposit receipts (TDR's) used by Great Britain except that the accounts were of indefinite maturity. The system worked well; as a result it was carried over into the permanent financial structure by legislation in 1945.

The reader of this book is forced to reflect ruefully that one of its major forecasts has already been proven wrong. Giblin concluded that high interest rates would never again be used as a device for restraint of credit. He adhered to this view, not for the reasons that are sometimes associated with the name of Keynes, but because he expected all central banks to be dominated by their respective treasuries and treasuries would universally want low rates.

This book is unfortunately marred by the lack of a subject index. The

chronological organization of the book requires a constant shift in attention among several subjects, such as the general economic scene, fiscal policy, monetary policy, and the state of external and internal banking reserves. This results in a choppy narrative style; a reader becomes easily confused with dates and events for which there is no ready remedy except a search of preceding sections. Nevertheless, the results are worth the work.

ROLAND I. ROBINSON

Northwestern University

Inflation. By PAUL EINZIG. (London: Chatto and Windus. 1952. Pp. 223. $2.50.)

Dr. Einzig has provided a stimulating contribution to the current literature on inflation. What he has to say is not as novel as he implies; nor is the rigor of the analysis likely to appear adequate at all times to the academic economist. Nevertheless, he is on the right track, in the opinion of this reviewer, and his argument is at least provocative when not compelling.

The author rejects what he calls the usual "static" definition of inflation as an excess of purchasing power over what is required by the attainable level of output at the current price level. In its stead he offers a more "dynamic" concept of inflation as "a state of disequilibrium in which an expansion of purchasing power tends to cause, or is the effect of, an increase of the price level" (p. 22); and which "is sufficiently pronounced and persistent to set into motion a spiral of rising cost-of-living, wages, cost of production, and monetary requirements and volume of money" (p. 23). What he considers to be novel in his definition of inflation is, I believe, the emphasis on price changes as the causal factor, with monetary expansion the result ("price inflation"), and on the dynamic interaction of monetary inflation and price inflation. Any upward movement in prices is likely to set off a continuous spiraling movement which the monetary authority is either unable or unwilling to check. Armed with this definition Einzig examines the relation to inflation of a wide range of circumstances and measures of policy. There is space here to mention only a few of these.

When the "undertone" (state of expectations?) is inflationary, increased taxation will not have a disinflationary effect, according to Einzig. Direct as well as indirect taxes will result in higher prices and income demands. These, in turn, will necessitate monetary expansion to sustain the higher price level. This is true even when the tax revenues are utilized to provide social services. For social service benefits tend to be considered as cost-free additions to income which are received as a matter of right. Consequently, income claims will rise in order to offset the income-reducing effect of the taxes levied. For this reason and because of a tendency toward over-full employment, the welfare state is inherently inflationary in Einzig's opinion.

The author effectively puts to rest certain naive notions about the disinflationary character of price increases. Surprisingly often in the recent debates over inflation one has come upon the argument that open inflation was preferable to suppressed, that the removal of subsidies on food prices would be

disinflationary or that the rise in the prices of imported goods was disinflationary because it mopped up purchasing power. These arguments, Einzig points out, ignore the dynamic nature of inflation wherein price rises lead to monetary expansion, to further price rises, etc.

His emphasis on price inflation leads him to reject the arguments of the "mild inflation forever" school. First, he doubts whether inflation with its dynamic character could remain mild. But, further, he suggests that price inflation may combine the disadvantages of both inflation and deflation. Price inflation is not employment-stimulating; rather, monetary expansion is required to follow it merely to keep employment from falling. Conceivably, continuous price inflation could accompany both underemployment and monetary stringency.

This no more than begins to touch upon the wide range of issues to which Einzig addresses himself with equally provocative results. There are comments on the post-Korean experience of the United Kingdom and a discussion of alternative policies for that country which, unfortunately, have been rendered less timely by the events of the more than one year which elapsed between the completion of the manuscript (as indicated by the date of the preface) and the publication date.

One shortcoming, in the opinion of this reviewer, is the failure to make explicit the assumptions which underlie the analysis. Whereas the traditional concept of inflation was adequate for a highly competitive economy where no problem of unemployment is recognized, Einzig's definition is tailored to a modern economy wherein competition is limited and the government is committed to the maintenance of full (or high-level) employment. It is only when "price makers" (to use Scitovsky's term) dominate markets that prices can be raised in advance of an expansion of purchasing power. And it is the full-employment commitment which is crucial in compelling the monetary authority to permit monetary expansion following on price rises; for the failure to do so would mean reduced output and employment, not reduced prices. This is implicit, I think, throughout the book, but its explicit statement early in the volume would have been helpful.

JOHN POWER

Williams College

La Monnaie. By ROBERT MOSSÉ. Bilans de la Connaissance Economique. (Paris: Lib. Marcel Rivière et Cie. 1950. Pp. 205.)

This book is the first of a series of "surveys of economic knowledge." The author undertakes to present a sketch of our knowledge of monetary affairs and ideas as they have developed in the last fifty years; "it is certainly not 'a digest', for it is directed towards a synthesis rather than towards an analysis . . ." Obviously to attain this goal in 130 pages (the rest of the book is taken up by Howard Ellis' Introduction, by the "Observations" of two other authors, and a bibliography) a careful selection was necessary; rather than to charge the author with omissions we should try to understand the viewpoint which guided him.

We notice first that the monetary theorist's dislike for problems of credit

institutions is not fully overcome; in discussing, *e.g.*, the controversies concerning the issue of banknotes, which filled the 19th century (p. 63), the author does not even mention the rôle of bills of exchange (or, more generally, of eligible paper) as collateral for banknotes.

Secondly, as far as monetary doctrine is concerned, the book virtually stops with 1932: theoretical problems arising in connection with Keynes' *General Theory* are not discussed at all, except for a few paragraphs about the multiplier in Chapter 3. This omission is the more serious, since this chapter, after briefly stating the quantity theory in Fisher's version, presents Aftalion's theory of monetary revenue (R) determining the price level (R = PQ). Mossé does not seem to realize that Aftalion's formula is not an explanation but an identity (p. 79: "attention is concentrated on the equilibrium between the monetary revenue designed for purchases and the total values of sales of goods and services"); nor that money does not even appear in the equation. Otherwise, he would have realized that it was precisely the achievement of the *General Theory,* by introducing functional relations, to relate income to the quantity of money, and to convert a mere identity into a set of determining equations.

Thirdly, even where the theoretical thinking of the past is summarized, brevity rather than clarity seems to have been sought. In discussing the quantity theory, Fisher's distinction between normal relations and transitional period is at least implicitly presented (p. 74), but no definition and analysis of the concept of the velocity of circulation of money can be found, except the statement that, according to the quantity theory, it is either "constant" or "always neutralized by the 'velocity' of commodities (see Marget)." (How shall the reader understand this?) In discussing the quantity theory in the Cambridge version (Chap. 3), the author overlooks the difference between the *ex ante* nature of the *k*-coefficient and the *ex post* character of the transactions velocity. In expounding the multiplier, Mossé asserts that "it is necessarily associated with period analysis," though the Keynesian multiplier is static, and Haberler's article of 1936, to which Mossé refers, deals with the Keynesian, not with the serial multiplier. In a footnote Mossé raises the question: "Is not the multiplier simply another name for income velocity?" though the former is a pure number and the latter has the dimension of time.

For these defects in the theoretical "synthesis," the reader is, to some extent, compensated by the vivid sketch of the state of monetary thinking before 1914, and, in particular, of monetary history after 1914. The American reader will be somewhat surprised by the stress laid repeatedly on the dispute between "metalism" and "nominalism," which fortunately did not infect Anglo-Saxon and Scandinavian economists. Mossé greatly overrates the influence of Knapp's "*étatistique*" theory of money; it was almost unanimously rejected by both German economists and German bankers; the paper-money inflation of 1914-23 was, as in other countries, the result of muddled thinking and indecision in an emergency, not of "nominalism." How, by the way, is the claim that metalism was generally accepted around 1900, compatible with the prevalence of a "nominalistic" quantity theory of some sort among Anglo-Saxon economists?

There are added to the main text of the book two sets of "Observations."

Those by Professor Triffin, of Yale, are mainly concerned with exchange-rate problems, which were excluded from Mossé's discussion. The Observations furnished by Professor Federici of Milan undertake to justify Mossé's neglect of Keynesian thinking, which "does not contribute anything basically new to the knowledge of the monetary process as it can be gained from a modern interpretation of Fisher's theory" (p. 133). Federici, however, read Keynes in his own peculiar way. Keynes is supposed to treat "as secondary and subordinate the function performed by money precisely if it is not spent" (p. 123); most of Keynes' readers had been under the impression that he assigns a rather exaggerated rôle to liquidity preference. "In the modern theory the monetary quantity is reduced to a secondary role if compared with the role played by the volume and proportion of expenditure" (p. 129). This is true for certain post-Keynesian applications of his theory and easily explainable from the high supply elasticity of credit money since 1933. In the *General Theory,* however, the quantity of money is treated as the only strategic parameter, whose changes govern those of the interest rate, of investment, income and employment.

Space forbids the further enumeration and discussion of details. Points mentioned must suffice to justify the reviewer's feeling that, despite some undeniable merits, this *bilan* has not attained its aim of being a synthesis of monetary theory.

Hans Neisser

New School for Social Research, New York City

Geld und Gesellschaft. By Wilhelm Gerloff. (Frankfurt am Main: Vittorio Klostermann. 1952. Pp. 288. DM 25.00.)

This is a valiant and interesting, but only partially successful attempt to develop a social theory of money. The author has covered much of the ground previously in other books, particularly in his *Die Entstehung des Geldes und die Anfaenge des Geldwesens.*

The major part of the book consists of a sociological and anthropological analysis of the origins of money. Its economic significance is discussed only marginally, and relatively superficially since the author's conclusion is that the origins of money are to be found in the social sphere. They have little, if any, relationship to economic necessity or even convenience. Money entered the economic area only late in its development; when it did, however, its importance grew immensely.

It is the author's thesis that the three major stages of the development of money are closely related to three human sources of cultural development: instinct (*Trieb*), habit (*Gewohnheit*) and reason (*Verstand*). The basic instinct, we are told, is the desire for recognition of one's achievements, and resultant social distinctions. This instinct causes the hoarding of valuables. The origin of money lies in its function as a hoarding device. From the habit of using money for this purpose developed the practice of using it as a means of payment for noneconomic functions, such as the purchase of wives,

expiation of sin, or the settlement of reparations. Reason finally turns it into barter-money and somewhat later into a tool for more general economic exchange. "Der Weg vom ritualen Geldgebrauch zum rationalen ist der Weg des Funktionswandel des Geldes."

Money thus had its origin in tribal customs, frequently of a magic or mystical nature. At first they involved the making of gifts and at a later stage the exchanging of gifts. The objects that were repeatedly used for this purpose because of their general desirability—usually luxuries, such as rings—slowly developed into money. They were desired, however, not because they gave the holder economic advantage, but personal recognition and social distinction. Economic man is a fiction; *homo ambitiosus* is vivid reality, and primarily explains the origins of money. This part of the thesis is developed at great length.

Barter is the crucial step which gave money economic significance. With the expansion of economic institutions, the use of money grew from a tool for barter to a general means of payment and pricing. This was not a simple causal relationship, for money in turn assisted enormously in economic expansion. Money finally reached its greatest height in the liberal capitalistic economies of the nineteenth century. As various forms of governmental controls are imposed upon these economies, money loses some of its functions, and declines in importance.

The author's attempt at a detached, anthropological approach does not always succeed. For example, from the proposition that "correct" money is "just" money he concludes that such money requires a monetary unit which maintains its value. Money which declines in value leads to spendthrift consumption, paralyzes the will to save, and thus endangers social welfare. This conclusion—mistakenly generalized—has considerable, but not absolute, validity. In America, for instance, it is quite possible future economic welfare requires a measure of such "paralysis," *i.e.*, an increased consumption function.

A great deal of the analysis is truly fascinating, but it is buried in an ocean of verbiage. This will trouble Americans considerably more than Germans; for the latter have been confronted with such writing in their social and economic literature for many years. It is repetitious, badly organized, and exceedingly verbose; much that is obvious is not only stated, but frequently repeated. Many a straw man is set up only to be knocked down successfully. The book deserves to be carefully edited and, together with some of the author's other writings in this field, should be made available to the English-speaking world.

HANS A. ADLER

Washington, D.C.

Money and Economic Activity—A Selection of Readings in the Field of Money and Banking. Edited by LAWRENCE S. RITTER. (Boston and New York: Houghton Mifflin Company. 1952. Pp. viii, 404. $2.95.)

An up-to-date book of readings in the field of money and banking has been

lacking in the United States since the late 1920's. In the decade of the First World War this need was met by Phillips[1] and in the following decade by Ivan Wright.[2] But during the 1930's and 1940's a hiatus existed. Thus, it remained for Professor Ritter to provide the first book of general readings as a supplement to single-text material for the 1950's.[3] This he has done in creditable fashion.

The book is divided into five parts, the first of which deals with "Money and Commercial Banking." It contains eighteen selections on the nature of money, banking, and a money economy; asset management of commercial banks; adequacy of bank capital; and the supervision of banking, including proposals for reform.

Part II, containing thirty selections, is concerned with "Central Banking." The topics covered are methods of credit regulation, factors related to member bank reserves, interest rates, war finance, and proposals for reform in monetary policy.

The next Part deals with "The Treasury and Monetary Policy," and includes twenty-two selections. The main headings are gold and the monetary system, monetary aspects of treasury operations, and central bank-treasury relations.

Part IV, "The International Economy," presents five selections on international finance, the balance of payments, gold movements, and the dollar shortage.

The final Part concludes with four selections on monetary policy and economic stability.

The book is on the whole well organized, with a thread of continuity from beginning to end, in so far as is possible in a collection from many sources. A few exceptions, however, detract from the organizational pattern. Selection 5 introduces multiple expansion of bank credit and the methods of credit control prematurely, and is repetitive with Part II, Chapter 6 on "Central Banking." Selection 6 on the par collection system would fit more logically in Part II on "Central Banking." Also, this reviewer feels the inadequacy of the space given to "Money and Commercial Banking" (Part I)—98 pages, compared with 165 pages on "Central Banking" (Part II), and 70 pages on "The Treasury and Monetary Policy" (Part III). The central problem of determination of the value of money receives no attention, and inadequate space is accorded bank loans and investments.

The individual selections are well chosen from a variety of authoritative sources and with an eye for different points of view. Large use is made of the wealth of material growing out of the Douglas Committee investigation and of publications of the Board of Governors and the Federal Reserve Banks. The editor introduces each selection with a helpful summary of the leading ideas.

[1] Chester A. Phillips, *Readings in Money and Banking* (New York, 1916) 845 pages.

[2] Ivan Wright, *Readings in Money, Credit and Banking Principles* (New York, 1926) 1,081 pages.

[3] This statement does not overlook *Readings in Monetary Theory* by a Committee of the American Economic Association (Philadelphia, 1951), 514 pages. By design this excellent collection of the best articles was directed to one segment of the field.

Teachers of money and banking should find Ritter's volume a welcome contribution to available teaching materials—especially in two-semester courses that devote considerable time to problems of central banking and monetary management.

G. W. Woodworth

University of Michigan

Business Finance; Investments and Security Markets; Insurance

Share Ownership in the United States. By Lewis H. Kimmel. (Washington: Brookings Institution. 1952. Pp. xi, 140.)

Share Ownership in the United States was prepared by the Brookings Institution at the invitation of the New York Stock Exchange. Lewis H. Kimmel directed the project. Its successful completion, however, was the result of the cooperative effort of thousands of individuals, business concerns, and financial groups. The study concerns the ownership of publicly owned corporations. Proprietorships and partnerships were excluded, as was ownership in family and closely held corporations. The sole test for the inclusion of a stock issue was whether there was sufficient public interest in such issue. It should be noted that publicly owned corporations are considered to be those which are owned by individuals in their own right, in contrast to government ownership.

The study is divided into two parts. In Part I the objective is to determine the nature and characteristics of *shareholdings.* Each listing in the stock transfer book is counted as one shareholding. If a person owned stock in ten different corporations, this was counted as ten shareholdings. The analysis of shareholdings is based upon usable data furnished by 2,991 corporations covering 3,954 stock issues. Of this number 1,022 were preferred stocks; 1,436 of the issues were unlisted. It is estimated that the present study covers about 25 per cent of all publicly owned stock issues.

Apparently this is the first time any study has included a classification of shareholdings by type of holder. Reference will be made, however, to only a few of them. The shareholdings of record, when classified by type of owner, refutes the popular notion that women own most of the nation's securities. The figures indicate that, while more women than men are holders of common and preferred stocks combined, men not only own a greater number of shares but the total market value of their holdings is greater than that for women.

Part I presents, apparently for the first time also, an analysis of shareholdings by states and geographic divisions. The heaviest concentration of shareholdings, both for common and preferred stocks, is in the state of New York, a fact attributable in part to the numerous holdings registered in the names of nominees, brokers, insurance companies and other types of financial institutions. Shareholdings of record are also classified according to size of business and type of business.

Considerable attention is devoted to an analysis of shareholdings registered in the names of nominees of banks and trust companies, as well as in the

names of brokers and dealers. The purpose is to show how such shareholdings are distributed among the several classes of beneficial owners. This is probably the first attempt to analyze these aspects of share ownership. The shareholdings of record for the reporting corporations are adjusted to show how they would appear if there were no shares registered in the names of nominees and brokers and dealers.

Part II analyzes the number and characteristics of share owners. It is estimated that 6,490,000 individuals in the United States are share owners in publicly owned corporations. The study reveals that owners are relatively more frequent among people 50 to 59 years of age than in any other age group; also that there is a direct relationship between share ownership and education. Also affecting share ownership, as might be anticipated, is the level of income.

With respect to occupation, share ownership is shown to be highest for administrative executives, about 45 per cent of these being share owners. Professional workers rendering personal services—doctors, lawyers, and others—rank fourth with 12 per cent. Share ownership is found to be relatively more frequent in families of one or two members, and the proportions of families and individuals owning shares is highest in the far western states—about 12 per cent for families and 6 per cent for individuals. Share ownership per family is found to be relatively highest in cities with a population of 25,000 to 100,000. It is estimated that the average number of different issues per individual share owner is 4.1. Also, more individuals own shares in manufacturing companies than in any other type of industry.

Life insurance, savings accounts, United States Series E Bonds, annuities and pensions, outrank publicly owned stocks as a form of investment. Following stock ownership as a form of investment is ownership in other government bonds, privately held stock, real estate mortgages and bonds, and corporate bonds.

The major reasons prompting individuals to acquire stocks are given as the desire for profit value appreciation (28 per cent), and a desire for income from dividends (22 per cent). Only 1 per cent of those questioned indicated they were motivated by a desire to hedge against inflation.

The study indicates that many people have a direct stake in American industry and business enterprise. Not only are there some 6,490,000 individuals who own stock in publicly owned corporations, but millions more have an interest through their ownership of bonds and other credit instruments, as well as indirectly through ownership of life insurance and savings accounts.

The book is brief (140 pages), and yet it covers the subject in a satisfactory manner. Each tabular presentation, and there are many, is well explained.

The inferences to be drawn from this study, however, will have to be those of the reader. The author sets forth the various estimates and the analysis of the data in a dispassionate manner. No moral is drawn as to whether or not a particular state of facts is "desirable" for our economy.

This study is probably the most comprehensive that has yet been made of stock ownership, either as to shareholdings or as to share owners. It provides a wealth of information as to the ownership of the corporate sector of our

economy and, while the book will be particularly valuable to those intimately concerned with this subject, it should be required reading for all who have an interest in the welfare of our economy.

LEROY A. SHATTUCK, JR.

University of Pittsburgh

International Economics

The Economics of the International Patent System. By EDITH TILTON PENROSE. (Baltimore: The Johns Hopkins Press. 1951. Pp. xv, 233. $4.00.)

The central purposes of this book are to examine the economic significance of the right of an inventor to obtain patent protection not merely in one country but throughout most of the world and to evaluate the precautions most frequently taken by governments against abuse of this right. Though Mrs. Penrose regards the economic arguments usually used in support of the patent system as open to serious question and qualification, she provisionally accepts them in order to focus attention upon the significance of the type of patent grant that covers inventions already patented and worked in foreign countries. The principle of such grants has been generally adopted, she finds, because of pressures from large industrial countries, exporting firms, patent lawyers, and spokesmen for so-called internationalism, in spite of persistent and often well-founded doubts on the part of various small and underdeveloped countries that their interests are served thereby.

Mrs. Penrose concludes that when the geographical scope of patents is expanded by licensing foreign inventions, there is little reason to expect a significant increase in the rate of invention or industrial development and that, for the world as a whole, any such social gains as may appear are likely to be outweighed by heavy social costs taking the form of higher prices and less efficient use of resources. Nevertheless, she thinks that an international extension of patent protection is necessary to cope with problems as to the location of industry that would otherwise be created by national patent systems. If an invention could be patented in only one jurisdiction, patents would tend to concentrate in the larger industrial countries, and, in so far as there were requirements that the patents be worked, plants would be constructed, not in the best locations, but in the patent-granting jurisdictions covering the largest markets and thus offering the largest potential profits from patent monopoly. Moreover, concerns not possessing the patent would erect plants in other countries in order to escape control by the patentee. Thus tactics related to patents might overcome economic considerations bearing upon industrial location, and the export trade of patent-granting countries might be reduced by the frequency of the incentives to produce abroad rather than deal with the patent-holders.

Two ways of reducing the social costs of the international extension of patent rights are analyzed by Mrs. Penrose. The first, compulsory working of the patent within the patent-granting country, she finds to be not only in-

effective but also conducive to an uneconomic multiplication of plants, some in undesirable locations. The second method, compulsory licensing, she considers an effective and flexible way of preventing most of the more serious restrictions upon industry, especially the monopolistic activity of certain international cartels and the withholding of new techniques developed abroad that are needed by domestic industry.

In developing her central argument, Mrs. Penrose relates it to the more important provisions of the International Convention for the Protection of Industrial Property. She concludes that although the convention is one-sided it cannot be blamed for the defects of the international patent system and that the effort to use it to eliminate working requirements has been sound. However, she recommends that its restrictions upon compulsory licensing be eliminated, that use of this device be encouraged, that nonindustrial countries be exempted from the obligation to grant patents upon foreign inventions, and that the possibility be explored of using the Convention to alleviate restrictions on trade and industrial activity incident to unregulated patenting.

This reviewer finds nothing to criticize in the execution of this book, except possibly that the argument is presented too compactly. Important questions are put, and clear and persuasive answers are given. However, the general plan of the book has certain defects. The chapter on the history of patent laws is only tangentially related to the central theme, and, though well done, is too brief to be useful to the audience to which the rest of the book is addressed. The chapter on the rationale of national patent systems summarizes and criticizes arguments on behalf of patent laws, but, having been conceived as introductory and therefore brief, it does not reach conclusions as to the effect of the patent system upon industrial development within a single country. For lack of these conclusions, the discussion of the effects of an international extension of patent rights has a provisional flavor. Its logical place is in the second volume of a two-volume work, but volume one is missing. It is to be hoped that Mrs. Penrose will supply it later. But in spite of this gap in the exposition, the present volume is one of the best among the few works concerned with economic aspects of the patent system.

Corwin D. Edwards

Washington, D.C.

Survey of United States International Finance, 1951. By Gardner Patterson and Jack N. Behrman. (Princeton: Princeton University Press. 1952. Pp. xi, 325. $2.25.)

With the appearance of its third report, it is hoped that the International Finance Section of Princeton University will no longer regard the publication of an annual summary on international economic issues of the United States as experimental, but will conclude that the survey has established itself along with the annual reports of the Economic Commission for Europe, Organization for European Economic Cooperation, Bank for International Settlements and governmental institutions. Unlike the ECE and BIS reports in that it eschews opinion, and differing from the OEEC and governmental writing which must

ignore the existence of differences of opinion even when taking a side of an issue, the *Survey* has won its place as one of the indispensable annual additions to the library of economists concerned with international economic matters.

The *Survey's* purpose is to record facts and issues. Unlike the ECE it is not interested in the imaginative use of statistics to make a point. Its statistical contribution is limited to the presentation of balance of payments data for the United States, slightly reworked to eliminate inconsistencies and for clarity.

Its discussion of issues, moreover, is not confined to "international finance" but ranges over a wider field into commercial policy, European integration, and Point Four. Finance in the narrow sense, in fact, is not a concern of the authors, for the chapter headings include nothing on the foreign exchange value of the dollar, nor do they discuss international capital movements in detail with the use of the weekly data. Entirely properly, the *Survey* is directed to finance in the broader sense.

Within this area, it is possible to take exception to the emphasis given by the authors to the events of 1951. The nationalization of the Abadan refinery in Iran and the collapse of commodity prices from the giddy heights reached in March of that year are both discussed only tangentially, though they cast their shadow far into the future in the United States' financial relations with the rest of the world. No mention is made of the United Nations' experts' report, *Measures for International Economic Stability,* although the other two UN reports by experts are referred to.

The usefulness of the *Survey,* already high, might be enhanced by the inclusion of an annotated bibliography of documents and articles bearing on issues. Much of this is already made available, but strewn through the footnotes, from which it must be exhumed via the index.

What must be admired is the authors' capacity to remain above the battle and to describe issues and even to characterize reactions but not to take sides. Most of the adjectives used are descriptive but involve no subjective evaluation. The nearest thing to a slip in this regard is the reference to an article by John Foster Dulles as "lucid." This departure from the high standard of aloofness otherwise maintained may perhaps be forgiven on Freudian grounds in a Republican year.

C. P. KINDLEBERGER

Massachusetts Institute of Technology

The Pattern of United States Import Trade Since 1923. By JOHN H. ADLER, EUGENE R. SCHLESINGER and EVELYN VAN WESTERBORG. (New York: Federal Reserve Bank of New York. 1952. Pp. 136.)

In this study, covering the period 1923-1950, the authors have investigated the U.S. demand for imports, compiling separate series for the main geographic areas of origin. For each area—with a few unimportant exceptions—there are value, price and quantity series for crude foodstuffs, manufactured foodstuffs, crude and semi-manufactured materials, and finished manufactures, as well as

for total imports. This detailed treatment is of particular help in studying problems that have a varying geographical impact, such as the effect of our business cycle on imports from the Sterling Area, and thus on the British balance of payments.

Two conclusions stand out. Confirming earlier findings, the authors agree that the level of economic activity and real income in this country were the primary determinants of our imports from each area.

The second, more surprising, conclusion is the estimate of price elasticity for finished manufactures, particularly those from Western Europe (the European Recovery Program countries). The authors believe that a change of 1 per cent in the relative[1] price of this category would change our imports (in the opposite direction) 2 to 3 per cent in volume terms. For many commodities, the price elasticity tends to be greater the larger the proportionate change in price; and it also becomes greater the longer the time period considered. This is contrary to the belief of many students, some of whom have felt that the elasticity for this group may even be less than 1.

Some of the other findings regarding price are similar to those for the domestic economy that F. C. Mills' work[2] has shown. Over the cycle, the greater the variation in price, the less the variation in quantity, and *vice versa*. Prices of primary imports fluctuate more than those of finished manufactures. Geographically, this means that the ERP export price structure is more stable than that of Latin America or the outer Sterling Area, but the ERP volume of sales, and thus employment, faces greater fluctuations.

The possibility of the price mechanism serving to facilitate the substitution of foreign for U.S. goods in the U.S. market is greatest for finished manufactures. Not only would the total volume of our imports of this group rise if their prices fell, relative to competing U.S. goods, but their relative share of our market would also increase.

As compared with the prewar period, our gross national product in real terms has risen more than the volume of imports. For raw materials and semi-manufactures this may indicate a change in the fundamental relationship to domestic activity, although the authors believe that for small year-to-year changes the prewar relationship still holds. For finished manufactures, however, it is thought that the relatively greater rise in foreign export prices compared with competing U.S. goods is a principal cause of the downward shift since prewar in the proportion of these imports to domestic output, and that a reversal of this price shift would restore the prewar ratio.

The postwar decline in the proportion of our imports coming from the ERP countries was noticeable also in the interwar period, and reflected the downward trend for finished manufactures imports in general. Part of the recent relative loss may have been caused by the fact that World War II stopped almost all imports from continental Europe, thus transferring U.S. demand to other sources (including domestic suppliers). But part of the shift can be

[1] Relative to the price of finished manufactures from other areas including those produced in the United States.

[2] *Cf.*, *e.g.*, *Prices in Recession and Recovery* (New York, 1936), Ch. III.

attributed to the great increase in our demand for durables, items which have always been negligible among our imports.

Within the ERP group the relative decline has been greatest for those countries whose export prices have risen the most, proportionately. The average price level for the group as a whole has also risen relatively to our prices; this, the authors believe, led to a shift away from ERP goods. Their conclusion, however, may not be the only important explanation; it is possible that the dislocations caused by the recent war curtailed supplies so much—particularly in the export trades, whose connections with this country had been disrupted for almost six years—that the mechanism worked from a shortage of goods to a price rise. In other words, the greater the shortage of a particular product, the greater the relative price rise, and the greater the relative fall in its share of our imports.

One assumption which may be questioned is with respect to the elasticity of supply of finished manufactures to the U.S. market. The authors assume almost perfect elasticity over the relevant range. It is true that goods can be shifted from the ERP home market if the suppliers so desire. But this may not be as simple as the authors imply. Even if prices are more attractive in this country, it may be difficult to establish sales outlets—in fact, in view of the vagaries of our market and tariff policies, it may not be worth the risk. For example, let us assume a strong demand for automobiles in this country. On a purely comparative price calculation it might pay to ship autos here rather than sell them at home or in other markets. However, there may not be the necessary repair and service stations to maintain the cars; there may not be the dealers' outlets willing to stock new—and, to the American consumer—strange products. Moreover, if the sales effort is successful, a tariff may be placed on this item, ending the imports and making the previous sales efforts worthless. Hence, the supply inelasticity may be quite high, particularly when the normal inertia supplements the other obstacles. So long as other profitable markets exist into which entry is less difficult, the greater profitability of the American market may not offset these other fears.

It is in the price policy implications that we find another possible weakness in the study, although one that is probably inevitable. Based on historical data, the study shows that a decline in the relative price of finished manufactures imports would increase the volume of sales so much that total receipts would rise. The obvious policy inference is for foreign manufacturers to reduce costs and prices. But what cannot be certain is the reaction of U.S. competitors. If the per unit margin is sufficiently large, our prices can be reduced to meet this outside threat; in other words, although a relative fall in foreign sales prices would boost imports, any serious inroad into domestic markets might result in sharp reactions by our own producers, even ignoring the further complication of pressure to increase the tariff. The only two widespread decreases in foreign finished manufactures' prices relative to U.S. competitors were those associated with the devaluations in 1931 and 1949. In both cases the income effects on sales were so strong that producers may not have been aware of the influences of the accompanying price changes. It will be recalled that 1931 was a year of almost steadily declining incomes and output in this country, while in mid-

1949 the recovery from the preceding recession began. If, in a future situation, foreigners reduce their prices relatively, and if it is clear that a successful penetration of our markets results from this policy, U.S. producers may react pricewise more vigorously. Hence, although a relative price reduction might aid foreign sales, in fact, it might also call forth corresponding cuts by U.S. firms, so that the gain to the foreign seller would thus be much less than is implied by the calculated elasticity.[3]

Another weakness of the study is the assumption regarding the constancy (over the relevant ranges) of the price and income relationships. In the study both arithmetic and logarithmic equations were employed. The former implies that the marginal propensity to import was assumed constant; the latter implied "that the income and price elasticities are constant throughout the entire range of observations and independent of the size of absolute changes" (p. 49). Yet, it may well be true that at different levels of national income, a given change in incomes will produce different changes in imports. In a depression, with much unemployment of domestic resources, increases in income may go mainly into the purchase of domestic supplies, while at near-full employment a greater impact will be felt on foreign supplies because of the tighter domestic supply situation.[4] Again, the impact on specific commodities, and thus on different areas, may also change with changing income levels. The 1950 upswing in U.S. economic activity after the Korean outbreak boosted certain imports much more than a similar upturn from a lower level would have done. Steel and chemical imports from Western Europe rose much more than would have been the result had the economy been at, say, only 75 per cent of capacity.

Implicit in the preceding criticism is the observation that a given change in national income (or GNP), even from the same absolute level, may not always have the same impact on imports. An upturn resulting from rearmament may so concentrate demand on, say, the domestic steel industry, as to result in capacity operations—and thus the need for more imports—at a national income level lower than an upturn resulting from government expenditures on public works. In the former, steel imports (and thus total imports) would rise more, proportionately, than in the latter case, even though the proportionate change in national income were the same in both possibilities.

It is exceedingly difficult—if not impossible—to allow for these various possibilities. Indeed, even if the functional relationships suggested could be deduced, the extra work required for the refinements and the more complicated mathematical equations resulting, would probably not be justified. Hence, these observations are to be interpreted as qualifications, rather than rejections, of the findings.

EDWARD MARCUS

Brooklyn College

[3] In fairness to the authors, it should be noted that they have recognized these two influences—price and income—and have employed calculations intended to separate them out. *Cf.* their study, pp. 50-51, 59; also, Ch. v.

[4] *Cf.* the case of interwar Canada in the study of E. Munzer, "Exports and National Income in Canada," *Can. Jour. Econ. Pol. Sci.*, Feb. 1945, XI, 35-47.

Business Administration

Economics of Business Enterprise. By LEONARD A. DOYLE. (New York: McGraw-Hill. 1952. Pp. xiii, 343. $5.00.)

Professor Doyle has written a text whose aim is to make marginal analysis the central theme and integrating principle of an upper division and graduate training in business. In particular he puts forward techniques that he believes would enable practicing businessmen to apply economic analysis directly to managerial decisions. In addition, Doyle tries to give the student the equivalent of a rigorous course in intermediate economic theory. His book is an unusual combination of topics, some of which are treated in an unusual manner.

The first two chapters are introductory, setting the discussion of the business enterprise system in a broad frame. Doyle describes the structure of the American economy through the device of an imaginary trial in which the private enterprise system is charged with suppressing competition and promoting monopoly. In the course of this trial, basic data describing the number, size, industrial composition of firms, profitability of corporate firms by size, national income variations, measurement, and sources are set forth. The use of the mock trial form helps to kindle interest in what might otherwise have been a dull presentation of data and statistical methodology.

The second chapter deals with two quite separate subjects. The first is the nature, problems, and structure of the firm. Combined with this discussion is a treatment of the rationale of a free enterprise economy, concluding with a crisp, penetrating discussion of the discrepancies between the rationale and the real economy. The contents of this chapter are excellent, though its organization leaves much to be desired.

There follow two chapters dealing with the nature of pure competition and demand, with emphasis upon the benefits to the individual businessman of escaping the rigors of competition, primarily by restricting entry. In explaining the operations of pure competition, Doyle works through an agricultural illustration, employing elaborate numerical computations as well as the routine geometric tools. The discussion of market demand is commonplace, but adequate.

In the second of these chapters entitled "The Behavior of Market Demand," Doyle discusses monopoly and imperfect competition, indicating briefly how they come to pass and their attractions to the businessman. The arrangement of topics is somewhat perplexing but the author's meaning is clear enough. Missing from this chapter is the basis for Doyle's conclusion (p. 330) that . . . ". . . the elasticity of demand *decreases* as the quantity is increased," which he says was explained in this chapter. The income pyramid described in detail by Doyle tempts one to infer, on the contrary, that the elasticity of demand frequently rises at lower prices and larger quantities.

As illustrative of imperfect competition, Doyle discusses the professions and retailing. His discussion of the professions is a cursory but interesting essay concerned as much with the socialization of medicine as with anything else. However, it contributes little to the author's fundamental objective and might have been omitted.

Doyle's discussion of imperfect competition among retailers is one of the more difficult parts of the book. It introduces some improvisations and variations on the break-even chart that this reviewer has not encountered before. In addition, it deals with various problems, among them "fair" trade laws, the mark-up method of pricing, and the reasons for diverse types of retail outlets. While this chapter covers important subjects and is anything but routine and unimaginative, it probably will prove troublesome to student and teacher alike because of the brevity with which major problems are touched upon. For the most part, the chapter sets up apparatus that will enable a retailer to decide how much it must sell at a given level of mark-up to break even and to cover its costs, including a target rate of profit. While retailers probably do think in terms of mark-ups rather than absolute price, the transformation of one into the other is hardly vital for basic understanding and might not justify the effort required to master it. Specialists in the subject of industrial pricing will, however, find it of interest.

In discussing the advantages of large-scale operation, Doyle covers more than is ordinarily treated under that head. He discusses at some length the determination of obsolescence costs and the suppression of new machines and methods. This chapter is careful and competent. The use of twelve involved diagrams, however, threatens real difficulty for even the best graduate students of business.

Like most other chapters, the one treating costs of distribution represents a collection of interesting and illuminating remarks combined under a framework of organization that is somewhat strained and novel. Much of this chapter is devoted to a discussion of vertical integration and the diverse movements of price and alleged differences in demand elasticity at various stages between raw material production and final purchase.

The most strategic chapter in the book is entitled "The Marginal Analysis and Practical Business Operation." It is here that Doyle elaborates the break-even chart, relating it closely to the marginal cost-marginal revenue equality of traditional price theory. In addition, reference is made to conventional business practice and the amount of information available to the average businessman. This section of almost thirty pages is tightly written. While no careful count has been made, the word "if" probably appears more frequently than any other, for supposition follows supposition and the reader is required to work through numerous successive examples. In effect, these examples require the reader to make some numerical calculation to determine whether revenue rises more than cost from a specific circumstance.

We learn in that chapter that the break-even and profit charts are the techniques that enable businessmen to apply economic analysis to managerial decisions. If I understand Doyle rightly, he concludes that economic analysis contributes to practical business operation by: (1) directing the decision-maker to ask whether a particular action contributes more to total revenue than it adds to total cost; (2) calling attention to the possible effect of output on unit cost; and (3) directing thought to the responsiveness of sales to price changes (p. 333).

The chapter which follows applies the break-even chart directly to decisions of multi-product firms, and completes the discussion of price and output decisions of the individual firm. Subsequent chapters are concerned primarily with general pricing practices and their social implications.

Chapters 11 through 13 deal with price leadership, uniform delivered price systems and price discrimination. These chapters depart little from the conventional treatment of these topics, though there are many statements that are not proved or shared by economists generally. They will not injure the advanced business student, however, for his prejudices are likely to run counter to Doyle's.

The last two substantive chapters attempt to explain the incomes of labor and management and discuss the interest cost and financial solvency problems of the individual firm. These chapters will do little more than afford an opportunity for teachers so inclined to introduce these topics. Apart from well-balanced statements about labor relations in general, the first of these simply makes difficult to comprehend the simple dictum that employers should not pay workers more than they contribute to total value of output. The final chapter, entitled "The Economics of Finance and Bankruptcy," touches on less familiar ground, but here again straightforward concepts are over-elaborated and probably obscured by the author's desire to say everything geometrically.

It is difficult to make general statements about a book that is so uneven in its separate parts. On the debit side, Doyle may have made a strategic error in making the book so short, or, equivalently, in deciding to do so much. Although his writing style is crisp and clear much of the time, the better-than-average student probably will find this book very difficult. Part of the student's difficulty will result from the book's organizational weaknesses. Especially in the arrangement of topics and sequence of ideas within individual chapters is improvement possible. The book also suffers from a shifting viewpoint. Business practices are discussed from the standpoint of owner profits and social benefit without clear indication of which viewpoint is being applied. Also, greater justification for the heavy reliance on the break-even chart than has been offered would be desirable. The reader waits in vain for discussion of the difficulties that are faced in applying it in concrete cases and for an examination of alternative methods that might be employed to obtain similar results. Finally, Doyle, as most other authors writing in this general field, makes numerous statements about business procedure or conditions that are unsupported. They are stated as if they were generally accepted conclusions, even when they are minority, though possibly correct, opinions.

On the credit side, Doyle undertook an eminently worthwhile task. The value of economic analysis for a practicing businessman has long been a major problem confronting college teachers and administrators. That this reviewer does not share Doyle's conclusion on this matter is irrelevant. Each reader can decide for himself whether or not Doyle has shown that refined economic analysis is an important aid to a practicing businessman. Doyle certainly has been intelligent, industrious and imaginative in trying to make economic

analysis relevant and useful to business operation. In addition, from my limited knowledge of the literature, Doyle has introduced refinements for break-even chart analysis that some will find useful.

The reader will almost certainly come away from this book with the feeling that it is intelligent, imaginative and sensible. Doyle clearly could write a lucid, original and illuminating book about business operation and decision-making. However, he did not quite reach the mark in this book.

A. R. OXENFELDT

The City College of New York

Marketing in the American Economy. By ROLAND S. VAILE, E. T. GRETHER, and REAVIS COX. (New York: Ronald Press. 1952. Pp. xviii, 737. $6.00.)

The appearance of this book was eagerly anticipated by many teachers in the field of marketing because of the recognized standing of its authors and because teachers were led to believe that a new approach to the subject would be taken. It does differ substantially from other widely used introductory texts in marketing.

The authors state at the outset that "We have a common meeting ground . . . in our conviction that students can best be introduced to marketing by a textbook whose primary point of view is the transcendent importance of this social institution as a vast and complex function of our free-enterprise economy." This statement together with all it implies to people trained in the social sciences serves well to characterize the book. Marketing teachers recognize that the subject can be approached in the manner suggested or primarily from institutional, functional, or managerial points of view. Inevitably there is something of each in any comprehensive treatment of the subject but primary emphases may differ and, in this instance, the emphasis is definitely upon the social and economic significance of those activities which have come to be included under the term "marketing."

The authors think of marketing as *the* directive force in a free-enterprise economy. Specifically, the two principal or basic tasks of marketing are stated as follows: (1) to direct the use of resources and allocate scarce supplies in conformity with existing demand, and (2) to aid in making consumption dynamic in conformity with changes in our ability to cater to human wants." Furthermore, the concept of "resources" is a broad one as it includes human as well as natural resources and "the political and economic institutional arrangements under which we live." It is likely that these broad concepts are foreign to the thinking of many teachers of marketing. Perhaps they will not find themselves in agreement with this conceptual pattern but at least their thinking will be stimulated by the ideas presented, particularly those in Part I under the title, "Marketing in Our Economy."

The book proceeds with Part II, Specialization and Integration in Marketing; Part III, Buying and Selling; Part IV, Pricing; Part V, Marketing within and between Regions; and Part VI, Marketing Efficiency and Control. Part II is particularly well organized. After an opening chapter on channels of distribution there are excellent chapters on collecting, sorting, and dispersing mer-

chandise and on organization of the flow of ownership. These are followed by a too short discussion of wholesalers and retailers. The authors then consider nonprofit agencies, nonowning agencies, and, finally, questions of stability and change in marketing agencies. In this latter chapter there is a short discussion of the historical development of marketing. While, to the reviewer, this is a most welcome addition to any introductory textbook on marketing, there is too little of it. Marketing students need more historical perspective. After reading many textbooks in marketing, students may well think that goods and services were first marketed about the time of World War I.

Part III on Buying and Selling comprises a number of very short chapters on buying, on selling of primary products, and on selling by manufacturers and distributors. They are too short to be reasonably comprehensive, particularly the one on selling of primary products. The marketing of farm products is given but scant attention anywhere in the book. The last chapter in this section, on nonprice competition, while good, again is too abbreviated, particularly in its treatment of aggressive selling as a nonprice weapon. A better balance of attention to subject matter according to its relative importance would have been gained if this part of the book had been expanded, with more attention to the activities of the manufacturer, the wholesaler, the retailer and, perhaps, other intermediaries as marketing institutions. This expansion would have overcome a possible criticism that the student may actually know little of such institutions after reading this text.

In Part IV the authors have compressed much on pricing and price policies into about one hundred pages. The topics and subtopics are well chosen and, while the discussion of each is necessarily limited, these chapters do serve well as an introduction to the whole field of pricing. Chapters are included on the mechanics and procedures of pricing, pricing problems and policies, pricing by primary producers and manufacturers, pricing by distributors, and price structures and the price system.

Marketing teachers will have mixed reactions to Part V, Interregional Marketing. While the material presented is important, of interest, and new to introductory marketing texts, the question arises whether it should appear therein in view of space limitations. Publishers apparently feel that textbooks of 700 to 800 pages are of "right" length. This imposes difficult problems of choice of materials upon authors. Furthermore, knowledge in this field of marketing has grown so tremendously in the past three or four decades that authors must, of necessity, give much attention to problems of inclusion and emphasis. Part V, to this reviewer, is marginal material for an introductory text, and it might well be condensed or reserved, in part at least, for advanced texts on marketing or for those on marketing research. If this material was condensed, more attention could be given to marketing history, to marketing institutions, to the marketing of agricultural products and raw materials, and to other subjects which are dealt with too briefly.

The final section, Part VI, Marketing Efficiency and Control, is well integrated and well related to other parts of the book. The authors discuss market research and information; budgeting and accounting controls; the social performance of marketing, with particular attention to the problem of measuring

efficiency; and government regulation. To a greater extent than in Parts II and III attention sufficient to satisfy the reader has been given to each subject covered. The nature of the attacks on marketing and the defenses against them and questions relating to the social significance of marketing are particularly well handled.

Teachers will find this book more difficult to use than other marketing texts. For its effective use the instructor will have to be better prepared and more competent in the social sciences. The concepts are so erudite, the materials presented so condensed that the student will need more explanation and discussion in class sessions than is usually the case. While the book has a most interesting and challenging approach, it may be questioned whether some teachers of marketing are sufficiently well prepared to use it effectively. Furthermore, it deviates so much from the texts most commonly adopted that the instructor may have to reorient his thinking on marketing. Still the use of this book will provide a stimulating educational experience for both instructors and students.

It might also be noted that the content of advanced marketing courses may have to be changed if this introductory text is used. The reason is that the instructor can not assume to as great an extent as with other texts that the functions and practices of market intermediaries are known by students, or that they have a thorough groundwork in all phases of marketing, particularly from an institutional point of view. Nor does the book lend itself well to a joint case-textbook approach in a course.

The foregoing comments should not be taken as critical ones but only as a recognition of the fact that the authors have seen fit to prepare a different type of book than has customarily been written in this field. There is penetrating analysis in this book, there are many bits of wisdom sprinkled on its pages, there is good organization of subject matter and much good writing. Moreover, in general, collaboration among the authors has been effective. But whether such a book as this meets the requirements of a good introductory text unless the first course in marketing is postponed to the junior or senior year will be questioned by many marketing teachers. Few will doubt that the book should be required reading by students of economics and by those of business administration at some time during their college years.

D. M. Phelps

University of Michigan

Industrial Organization; Public Regulation of Business

Defense, Controls and Inflation. Edited by Aaron Director. (Chicago: University of Chicago Press. 1952. Pp. x, 342. $3.50.)

In April 1951, the University of Chicago Law School sponsored a conference designed to "clarify the areas of agreement and disagreement" concerning economic controls during the post-Korea mobilization. Attending were 70

persons selected from business, law practice, journalism, labor organizations, legislative and administrative posts in government, and members of law and economics faculties. Working from a prepared agenda, separate sessions moved from monetary policy to fiscal policy, then to direct controls and on to the special problems Britain faces, and to the long-run effects of alternative control programs on free institutions. The verbatim report of the conference, introduced by a summary chapter by the chairman of the planning committee, constitutes the volume reviewed here.

From the opening session to footnote dissents to the one-chapter summary of the discussions, two sharply divergent policy programs were argued. One which was labelled that of the "Chicago School," consisted solely of vigorous monetary control carried out by Federal Reserve open market operations. No direct controls over wages, prices, or uses of materials, or over total amount or direction of use of investment funds should be imposed. Even a balanced budget, while helpful, would not be fundamental, for a deficit could be offset by still tighter money and the restricting effect of higher interest rates. Dissent from this extreme viewpoint was sharp and brought out an alternative program. The fundamental points of this plan were to place fiscal policy in the active rôle, with its effects reinforced by a marked, but not drastic, tightening of bank credit, and by varying degrees of direct control over investment and over the use and prices of scarce materials. Few argued for the general wage and price controls which were in force at the time.

These sharply different policy proposals are not new but the discussions of them provide an opportunity to reflect on the rôle of economists in one or more of the following steps in policy formation. (1) Clearly the economist is best qualified to provide, and is responsible for, the delineation of the underlying general relationship(s) in the area(s) of economics in which the problem falls. (2) Likewise, this reviewer contends, the economist must adapt such general theory, or blend it with other relevant theory, so as to develop the economic aspects of policy which the peculiar attributes of the particular problem require. (3) The further adaptation to what is legislatively and administratively feasible is the final step but one for which the economist has no particular competence. His counsel on probable economic consequences of alternatives may nevertheless be of value.

The proposal for sole reliance on interest rates as a mobilization period control does not go beyond step one. It is a literal application of the general doctrine that the price system is an efficient means for allocating resources. A business boom, a mobilization period, or full-fledged war all call for the same prescription. No attempt was made to show how, under the existing circumstances, resources would be reallocated with the speed and in the direction needed. Rather appeal was made to the logic of the price system, or how bad the alternatives are. On the specific point of whether interest rates could control inflation dissent was vigorous, but to criticisms of the plan the repeated response was in the vein of the poet's mountain climber, "Excelsior." Interest rates of 12 per cent would not bother Hayek (p. 59), for example. Such reasoning led Harrod to observe, "I should like to say that I do not think it is

an issue that can be decided by logic but rather by history and experience, and my contention is that history does not show that a tight credit policy can prevent the development of inflation in the kind of situation where there is a real demand for goods and services such as we have at the present time" (p. 64).

In like manner, but aimed directly at the logic of the "Chicago School's" position, was Hitch's observation that the theory which demonstrates the superiority of "free markets and the price system is a theory of 'comparative statics.' It shows that in a quite technical sense we can get an 'efficient' allocation of resources by using free markets in certain circumstances. But the method of analysis used is comparative statics. I know of no satisfactory theory which shows either that the path which the economy takes from one position of equilibrium to another is an optimal one or that, when time is important, we traverse that path at the optimal of speed" (p. 218).

Economists may debate whether existing theory gives insight into the character of the inter-equilibrium path, but that need not be resolved here, for it is speed and accuracy of readjustment of resource use which is fundamental. The risk of major errors cannot be taken for national security is involved. It was not economists at the conference who pointed out the irrelevance of Jewkes' argument that reconversion after the war in the United States and Germany showed how rapidly free markets could reallocate resources. Instead it was two lawyers and Senator O'Mahoney who indicated the fundamental difference between attracting resources temporarily from their long-term use, which is the mobilization problem, and releasing them to return to their long-term use, which is the reconversion process.

Equally important is the time pattern of developments during the mobilization period and the speed at which various controls can become effective, or cease to be necessary. That there had been the sudden impact of the Korean action, that there was under way a sharp buildup to a probably long-sustained higher plateau of military expenditures, got into the discussions only incidentally. What these facts meant for or against particular control programs or their duration does not stand out in the proceedings.

That the discussions did not deal systematically with such topics reflects the fact that the conference did not start with a consideration of the unique aspects of the mobilization problem. Had this been done, attention would have focussed quickly on step two mentioned above. That this was not done indicates once more the viewpoint of those who prepared the agenda for they are of the "Chicago School." To them mobilization is not or should not be dealt with as a unique problem. That is the logic of their position, although that logic is reinforced by step three considerations to the effect that controls may damage the free market system permanently.

Proponents of the composite program, which would include at least some direct controls, also had their views of political desirability. These included political inacceptability of very tight money and of falling prices and wages in sectors of the economy from which demand shifted. Consequently the monetary program would be ineffective and inflation substantial and real damage would be done to free institutions.

In noting the position taken by participating economists, one finds a fairly high, but not perfect, correlation between the extent to which individuals had been involved in public policy formulation and the program they supported. In general those who had had active rôles in World War II or Post-Korea mobilization, or had participated in the policy studies by such groups as the Committee for Economic Development or the RAND Corporation, favored the composite program which included at least some use of direct controls. Does the view of economists in these positions reflect a bending to political expediency, or a greater awareness of the unique problems of a mobilization period? Or do those who adhere to monetary policy only, or even that plus active fiscal policy, understand the economics of the situation more clearly; or are they blind to essential conditions? The reviewer frankly is of the former school and suggests that the "discipline of responsibility" aids in the formulation of, but should not dominate, economists' views on policy. When the lay participants in the conference referred to the "highly abstract" character of much of the discussion, this was their way of asserting that the proponents of the exclusive use of tight money had not adapted their analysis of the market system to the facts and forces of a mobilization period.

Richard B. Heflebower

Northwestern University

Grössenordnung und horizontale Verflechtung in der Eisen- und Stahlindustrie der Vereinigten Staaten, Grossbritaniens, Frankreichs, Belgiens, Luxemburgs und Deutschlands. By Herbert Steiner. (Kiel: Institut für Weltwirtschaft, Universität Kiel. 1952. Pp. v, 119.)

This very well documented publication investigates the relative size and horizontal integration of the iron and steel industry of the six countries mentioned in the title of the book. The relative size of the firms is measured in two ways: first by comparing the volume of output, and second by comparing the amount of capital investment. The relatively greatest concentration of economic power is found in Luxemburg where in 1950 the leading concern (ARBED)produced 64.5% of the raw steel. Next in line was Germany (Vereinigte Stahlswerke, 38.7% for 1938), the United States (U.S. Steel Corporation, 32.5%), Belgium (group Cockerill, 32.1%), France (group de Wendel, 19.5%) and Great Britain (United Steel Cos., 13.1%). The schedule of capital investment does not completely parallel the above sequence. In France and Great Britain the top producers are not the ones with the largest capital investment.

Much more interesting than the above, purely statistical, survey is the part of the publication dealing with horizontal integration. Here, the author shows the basic difference between industrial organization in the United States and in the European countries. The concentration and growth of heavy industry in the former has taken the form of holding companies (U.S. Steel and Bethlehem Steel) or outright mergers (Republic Steel) rather than of loosely organized concerns based on mutual shareownership and interlocking directorates customary in Europe.

Unfortunately the author gives us no information at all of the horizontal integration in Germany prior to May 1, 1952, the date on which the disintegration of German heavy industry by order of the Ruhr Authority was completed; nor does he draw any conclusions concerning the effects of horizontal integration on output and price formation.

HERBERT C. WITTGENSTEIN

Stanford University, California

Public Utilities; Transportation; Communications

Atomic Power—An Economic and Social Analysis. A Study in Industrial Location and Regional Economic Development. By WALTER ISARD and VINCENT WHITNEY. (New York: Blakiston. 1952. Pp. xi, 235. $4.75.)

Highly significant public policy issues may arise during this session of the Congress as the nation's legislators seek to clear the statutory way for future industrial development of electric energy from nuclear sources. Looking beyond these domestic policy problems, Isard, an economist, and Whitney, a sociologist, have joined forces in a successful interdisciplinary effort to analyze the more distant influence of nuclear power upon the location of large fuel- and energy-consuming industries and upon the development of industrially developed and undeveloped economies. The study follows by a year and a half the rather similar one prepared, under the direction of S. H. Schurr and J. Marschak, by the Cowles Commission.[1] Both are useful preliminary explorations into uncharted territory, that alluded to in Section 7(b) of the Atomic Energy Act of 1946. This directs the U. S. Atomic Energy Commission and the President to submit to the Congress a report, with legislative recommendations, setting forth their "estimate of the social, political, economic, and international effects" when the industrial, commercial, or other nonmilitary use of atomic energy has been "sufficiently developed to be of practical value."

The Cowles Commission study made rather optimistic assumptions concerning the future cost of nuclear power, but after surveying a number of large fuel- and power-consuming industries found that the probability of substantial savings in cost appeared small, although locational shifts might be facilitated by nuclear power in special circumstances. The findings of Isard and Whitney are not essentially different, although the method of analysis diverges in certain essential aspects, to be mentioned later.

At this stage of our knowledge Isard and Whitney are compelled to speculate on, rather than to measure, the expected impact of future nuclear power utilization. They first review the open literature on reactor technology (Part I) although the point of doing so is hardly made clear in economic terms. While recognizing that military needs during a long period of international tension are likely also to promote technical progress for peaceful applications, they

[1] *Economic Aspects of Atomic Power,* An Exploratory Study Under the Direction of Sam H. Schurr and Jacob Marschak (Princeton, 1950). Reviewed in this journal, December 1950, XLI, 994-95.

fail to appreciate the significance of possible dual-purpose nuclear reactors, producing as joint products power and fissionable material. After reviewing (in Part II) the scanty literature available on estimated nuclear power costs, practically all of it enthusiastically inspired during the first year after Hiroshima, Isard and Whitney take a skeptical attitude toward *specific* unit cost estimates, notably those of Schurr and Marschak, derived from the so-called Thomas report (1946). They quite properly conclude that no one can yet "really predict even a meaningful range of costs of atomic power at future stages of development" (p. 25). They prefer instead to think in terms of the levels to which nuclear power costs must fall to affect production costs, location of industry, or the introduction of new processes. The weakness of the specific cost approach can be readily demonstrated. If the cost of electric power from nuclear sources is dependent in part upon the "price" of by-product fissionable material, then the specific cost of electric power becomes indeterminate.

The direct cost-saving effects of nuclear power, Isard and Whitney proceed to show, are likely to be small, noting that purchased energy and power for United States manufacturing industries have never exceeded 7 per cent of the value added by manufacture. They look momentarily at possible indirect effects, concluding that Leontief's input-output analysis of interindustry relations may hold some hope of providing a method of measurement. After analyzing the theory of industry location, the authors state that "any lowering of power costs based on nuclear energy will hardly be of sufficient importance to nullify other locational forces. Any wholesale relocation of industry is unlikely" (p. 71). Case studies are made of the cement, glass, iron and steel, and aluminum industries.

In Part III the authors assess the impact of nuclear power upon economic development and upon regional economies, recognizing the contributions of such sociologists as W. F. Ogburn, H. Hart, and S. C. Gilfillan, who have stressed the cultural factors impeding or facilitating technological innovations. Isard and Whitney find that utilization of nuclear power, as for other innovations, will be determined by the conflict between "cultural resistances" and the effective need for low cost power. Thus, in industrially undeveloped nations cultural resistances and lack of real demand for energy resources may combine with the shortage of capital and skilled personnel to prevent significant utilization of nuclear power. An interesting case study is made of Brazil, an essentially agricultural economy deficient in both petroleum and coal. Power, they conclude, is unlikely to be the key to the economic growth of such industrially undeveloped nations.

Turning to industrialized areas, the authors suggest that the "stage of industrialization" may greatly influence the contribution of nuclear power but the brief for this seems hardly convincing. There is no disputing the fact, however, that while mature economies may possess requisite capital and technical resources, perhaps the controlling factor will be the extent to which aggressive industrial leadership, possibly assisted by government, will feel impelled to undertake nuclear power development. After comparative study of a number of highly industrialized countries, Isard and Whitney somberly conclude that

"among major nations, the United States appears least likely to improve its *relative* economic status through atomic development" (p. 187). Their final conclusions are realistically restrained. They foresee major cost savings as unlikely, and expect only limited commercial use of nuclear power. They concede that there may arise new industrial processes and a host of indirect effects not now discernible. This study is not the last word on a speculative subject, and the authors would be the last to suggest that it is.

Having benefited by two useful exploratory investigations, economists may now well ask, "How should we, and other social scientists, usefully proceed to analyze the advent of nuclear power?" Enumeration of the topics both studies failed to explore may be helpful. First, neither study assayed what small savings in power-generating costs might mean to the electric utility industry as such, having considered electric power consuming industries only. It is in the electric utility industry that nuclear power must make its economic way. Second, both studies were preoccupied with conventional industrial applications of relatively low cost electric power, and avoided consideration of specialized applications where nuclear power, because of its special technical characteristics, may be a peculiarly attractive and economical alternative. Nuclear power plants may ultimately have special advantages either as small "package" plants (for use in isolated areas, in ships and cargo planes) or as large complexes, exceeding 1,000,000-kilowatt capacity, located in high cost areas of heavy consumption. Third, one would hope that a careful analysis will be made of the nation's long-term need for nuclear power weighed against the vast conventional energy resources likely to be available over the next half century, but with rising fuel costs pushing electric power rates higher. Does industrial nuclear power on economic grounds warrant a major national effort? Finally, and most important perhaps, it is about time that economists and other social scientists began to direct special attention to the immediate issues of national economic policy impinging on the introduction of nuclear power for industrial purposes.

The U. S. Atomic Energy Commission recently announced that four industrial groups had submitted comparatively optimistic interim reports on the industrial development of dual-purpose power reactors. Such reactors were said to be technically feasible; the problems remaining are essentially economic and political. Should it prove possible to declassify these reports for public use, economists will have available for the first time technical and economic data needed for fruitful analysis of long-range implications, as well as imminent economic problems.

Here are some of the notable issues appearing, as the present provisions of the Atomic Energy Act of 1946 become impaled on the emerging facts of industrial nuclear power development: government monopoly is provided in the Act; should a new public policy be established permitting private ownership of nuclear power plants? How can the development of nuclear power be made consistent with the overriding requirements of the national defense? Should fissionable material needed to fuel civilian reactors be diverted from military uses? Should the federal government guarantee a long-run market for industry-produced plutonium? What rôle should such federal agencies as the TVA

and the Department of the Interior play in the industrial development of nuclear power? These and many other questions are now arising to challenge economic analysis in the formulation of public policy.

PHILIP MULLENBACH*

* The author is on the staff of the U. S. Atomic Energy Commission. The views expressed are those of the author and should not be considered as representing the views of the Commission or its staff.

Transportation: Principles and Problems. By T. C. BIGHAM and M. J. ROBERTS. 2nd ed. (New York: McGraw-Hill. 1952. Pp. xvi, 710. $6.00.)

The present volume, which is a revision and enlargement of a well-known text on transportation, is similar in scope to other texts in this field, but, as might be expected, differs in emphasis and organization. Since it is of moderate length and somewhat shorter than its leading competitors, some topics inevitably are treated very briefly. Opinion will doubtless differ widely as to the wisdom of the authors' allocation of the available space. The reviewer's opinion is that most subjects receive adequate attention and that in general the present volume is an improvement over the first edition. The most significant improvement, in his view, is the addition of chapters entitled "Transportation and Production" and "Transportation and Exchange." These cover the important subjects of the effect of freight rates on the location of industry and the relation of freight rates to the prices of commodities.

On the other hand, the reviewer feels that somewhat fuller treatment of a few topics would have been desirable. One such topic is transportation cost, which is good as far as it goes but which might well have included some discussion of empirical studies of cost, such as the rail and motor cost studies prepared by the staff of the Interstate Commerce Commission. A second topic which, in the reviewer's opinion, deserves somewhat more space than the nine pages allotted to it in this volume is public policy with respect to control over entry, especially as regards motor and air transportation. Third, the legal obligation of carriers with respect to such matters as acceptance of shipments, supply of equipment and liability for loss and damage would seem to merit at least a substantial portion of a chapter, whereas only about one and one-half pages are devoted to them. Finally, one other general topic which might well have warranted further attention is the organization and procedure of the federal transportation regulatory agencies, proposals for the reorganization of these agencies, and the relation of regulatory agencies to the courts and to the executive branch of the government. In contrast more than half of the chapter on the general level of rates (32 out of 58 pages) is devoted to the question of valuation and rate of return, an allocation of space which seems somewhat excessive in view of the relatively subordinate rôle played by these matters in discussions of transportation policy at the present time.

The general plan of organization of this volume is to treat successive topics "across the board" with reference to each agency of transportation instead of giving a complete treatment of each agency of transportation in turn. The reviewer expresses no opinion as to the respective merits of these two general

approaches but he would recommend at least two changes in the organization of individual topics. One of these is in connection with the treatment of transportation cost. Chapter 6, entitled "Transportation Costs: Economic Basis of Rates and Regulation," discusses constant and variable cost, common and separable cost, discrimination and ruinous competition; while Chapter 12, entitled "Principles Underlying Particular Rates," considers the economies of full utilization, long-run decreasing cost and value of service. The purpose of the authors in thus separating the treatment of these topics is apparently to make Chapter 12 serve as an introduction to the succeeding chapters on rate structures and the control of discrimination. The reviewer is of the opinion, however, that considering the close interrelationship of the topics mentioned there would have been a clear advantage in treating them together in the earlier chapter.

The reviewer's other principal comment with regard to the organization of the volume is that a more unified treatment of the problem of transport coordination would have been desirable. The volume contains much good material on this subject, notably in the chapter on interagency rate relations, but it might be difficult for students to obtain a comprehensive view of the various aspects of this intricate problem because the various aspects are not drawn together and related to one another in a single chapter or series of chapters.

The authors' views regarding transportation policy are conveniently summarized in the concluding chapter. Among other things they recommend that the declaration of national transportation policy in the Interstate Commerce Act be broadened to embrace promotion as well as regulation and to include air transportation in addition to the agencies presently covered. They favor centralizing regulatory authority over all forms of transportation in the Interstate Commerce Commission, maintaining the status of the Commission as an independent agency, and centralizing promotional functions in a Department of Transportation or in a division of the Department of Commerce. They present in some detail recommendations for the relaxation or liberalizing of regulation in some directions and for the extension of the scope of regulatory authority in others.

With a few changes in the order of assignment of chapters along the lines suggested above, and with the introduction of supplementary material at a few points, the reviewer believes that this volume should prove to be a satisfactory text for a general survey course in transportation.

Robert W. Harbeson

University of Illinois

Economics of Transportation. By Russell E. Westmeyer. (New York: Prentice-Hall. 1952. Pp. viii, 741. $6.00.)

A writer of a text in transport economics must necessarily select an emphasis, for the volume of material and problems to be described and analyzed in the field is too large to permit complete treatment. In his approach, Professor

Westmeyer has emphasized the public utility characteristics of transportation and the railroads among the major agencies. Thus, the text will appeal most to those who desire to give primary attention to the history and development of regulation and to railroad problems. Notwithstanding, considerable discussion is found of problems in air, motor and water transport, although subsidy and its effects upon the railroads receive the most thorough analysis; and some problems, such as the economics of highway investment, are scarcely mentioned. Freight forwarding, railway express, parcel post and Pullman services are discussed briefly, but many will regret elimination of reference to international shipping, the rôle of the automobile, and intracity transport.

The text is somewhat long for a one-semester course in transport economics, but with supplementary readings in periodicals and monographs, it could additionally be used in a transport problems course. It is readable and the exposition is clear. The author desirably avoids use of too many details of interest only to technicians and offers students the aid of numerous background explanations. The general tone is objective. Many economic problems are raised for discussion, with little tendency to express definitive judgments. Two methodological features open to criticism are the slight use of maps and charts showing facilities, traffic flows and the relation of rates to distance, and the infrequent reference to the best periodical literature. While other texts are cited frequently as suggested readings, the limited references supplied will not stimulate inquiring students to read more thoroughly in the appropriate theoretical works and in special monographs. However, helpful footnote references to leading regulatory decisions are found in chapters on regulation.

As with most transport texts, this one is heavily descriptive with respect to transport development, regulation, and rates and services. Although general statements of the nature of the transport market as a whole, the incidence of freight rate changes, the theory of transport demand, and the theory of rates are not found, much pertinent analytical comment appears throughout the book in reference to particular problems. Such analysis is usually clear-cut and competent. Nevertheless, many readers will regret the lack of a unifying frame of reference. As one proceeds through the chapters to the final two devoted to general transport problems and policies, one becomes increasingly aware that the author is concerned with an economic allocation of resources in transport. Yet Westmeyer does not inform the reader of that dominant theme at the beginning, nor does he comprehensively develop the theory and requirements of economic allocation. Much of what disturbs him about the resource allocation depends upon his assumptions of a normal oversupply of transportation, excess capacity in the railroad industry, and a high ratio of constant costs in that field. The economies of utilization and highly variable profitability in railroading are frequently mentioned in calling attention to the pessimistic consequences to the railroads and the public of the growth of other agencies relative to the railroads; of development of facilities in each agency independent of the effect upon other agencies; of the opportunity to operate which is granted to limited common carriers and to contract, private and exempt carriers; and of the excess transport supply resulting from subsidies to air and water trans-

port. And while the criterion of the position of the railroads is applied throughout, far less attention is given to the benefits of technological and service innovation, the advantages of reasonable excess capacity, the relation of transport to economic development, and the economics of investment. Does Westmeyer think a repetition of the restrictive tendencies of the 1930's in transport regulatory policy desirable? Some transport economists thought that the last war had ended the myopia of depression years in regard to adjustment of supply to demand!

Since transport economics basically must be concerned with allocation of resources, any text must examine the economic effects of promotional and regulatory policies upon the railroads. Those concerned with wastes in transport and continued private ownership of railroads will heartily endorse Westmeyer's frequent reference to evidence of the wasteful effect of subsidized transport. Admittedly, removal of subsidies would improve the allocation of resources, although Westmeyer appears at several points to exaggerate the benefits to railroads. But aside from measures to reduce subsidy, it is not shown how further coordination, which is described in confident and glowing terms, can be accomplished. Coordination is a nice word but its real possibilities cannot be developed without serious consideration of advantages of uncoordinated transport where subsidy has been removed and without close analysis of the workability of competition in transport. Neither is treated adequately in the book. The disbenefits of further control of interagency competition, a policy strongly implied, must be realistically appraised, not just mentioned and glossed over. And it is questionable that economic coordination can be evaluated solely in terms of high constant costs in railroading.

Despite the text's concentration upon regulation in each transport field and substantial exposition of regulatory policies, little attempt is made to develop comprehensively the economic results, positive or negative, which regulation and regulatory agencies have accomplished for the public. This is a common fault of most transport texts. Since transport functions through regulated markets, there appears an uncritical tendency to accept the regulatory structure and merely to describe key policies. While this may be informative, Westmeyer's statement of accomplishments of regulation in terms of the number of application and other cases which the Interstate Commerce Commission and the Civil Aeronautics Board have successfully handled is not very meaningful. It is not the process of regulation which is ultimately important, but whether it does, in fact, stimulate investment, innovation and invention; improve the allocation of resources; and work towards lower real costs. However, Westmeyer can be commended for drawing attention to some particular regulatory policies, such as certificate restrictions, which merit examination from the standpoint of end results.

The text is teachable and a useful addition to the growing alternatives available to those who offer courses in transport.

James C. Nelson

Washington State College

Industry Studies

Competition in the Rayon Industry. By JESSE W. MARKHAM. (Cambridge: Harvard University Press. 1952. Pp. xvii, 245. $5.00.)

Professor Markham's analytical study is a useful addition to the literature of industrial organization and behavior. The principal sources of his factual information were the trade journals and Federal Trade Commission hearings, but the author has also had personal experience in the industry and has enjoyed the cooperation of some of its members in estimating the relationships between cost and size. He successfully explains the structure of the industry and its behavior in terms of its underlying technology and of its historical and institutional background and then proceeds to evaluate its performance according to criteria of competitive workability.

The rayon industry in the United States dates from the establishment of the American Viscose Corporation as a subsidiary of Courtaulds, Ltd. in 1910. Patent protection and requirements for technological "know how," even in the absence of patents, acted as barriers to entry and limited success in the early years to subsidiaries of established European producers or to companies which could secure their help. The rayon industry thus was highly concentrated from the start, and marked cost advantages to size have kept the number of firms small. There are now 16 firms, 4 of which do 75 per cent of the business.

The products of all firms are identical grade for grade which suggests that in a concentrated market, price tactics must be either cutthroat or cooperative. Among other influences, common historical links to members of the European rayon cartel have encouraged cooperation. Cooperation has been achieved through price leadership, which Markham identifies with Stigler's "dominant firm" and "barometric" types, and through outright collusion on at least one occasion.

The cooperative interest in price is restrained, however, by two other market factors: the competition of other fibres and a strong incentive on the part of each firm to maintain capacity production. With technological progress rayon fibre has become an increasingly close substitute for other fibres in an increasing number of uses, with a consequently high cross elasticity of demand. The industry also believes that stable rayon prices are attractive to the fibre-using branches of the textile industry. Thus competition of other fibres both limits the level of rayon prices and encourages their stability.

The limit to the level of prices is reinforced and the stability of prices is threatened by the interest of each producer in capacity production. Each plant at any point in time has an inflexible upper limit to output while unit and marginal costs rise if output is curtailed below this level. Seventy-five per cent of capacity is a critical level below which costs rise sharply. Thus if sales decline, production is maintained for a time and inventories rise. With a prolonged decline, output must be cut and costs rise. The smaller firms, owing to higher cost levels, are most seriously affected, and if the cut is severe, they are likely to shade prices in secret while list prices, which usually are the actual prices, remain identical for all producers. If output is curtailed more

than 25 per cent the price leader is likely to reduce list prices thus reestablishing uniformity and enabling rayon to compete with other fibres on better terms. In this way a cooperative solution is found to the conflicting aims of price stability and capacity operation under conditions of fluctuating and highly elastic demand.

The net results of this market pattern have been rapidly changing technology, rapidly growing production and steadily falling price. In the early days when rayon producers had an effective monopoly of an inferior product with few close substitutes, the profit rate was high. By the late 1930's with a vastly improved product competing closely with other fibres over which rayon producers had little control, the profit rate on investment was no higher than the average for manufacturing corporations. There is little evidence either of output restriction or of price rigidity at the expense of output.

Markham concludes that the industry is workably competitive and suggests no revision of its structure. He observes that rayon has enjoyed a sheltered market as a result of absolute tariff protection since 1930 and as a result of the support given to cotton prices by the agricultural programs. The gradual removal of tariff protection is suggested.

This reviewer finds little reason to differ with Markham's analysis of the industry on any important issue. My principal complaint is against a generally awkward organization. As one example, Chart 11 which is first discussed on page 49 does not appear until page 150. To some extent this type of thing is a matter of personal taste and at the most detracts from the pleasure and ease of reading. It does not prevent this from being a good, convincing and enlightening book.

RICHARD B. TENNANT

New Haven, Connecticut

Land Economics; Agricultural Economics; Economic Geography

Resources for Freedom. VOL. I: *Foundations for Growth and Security.* VOL. II: *The Outlook for Key Commodities.* VOL. III: *The Outlook for Energy Sources.* VOL. IV: *The Promise of Technology.* VOL. V: *Selected Reports to the Commission.* A Report to the President by the President's Materials Policy Commission. (Washington: Supt. Docs. 1952. Pp. viii, 184; x, 210; viii, 43; x, 228; x, 154. $1.25; $1.50; 50¢; $1.75; $1.25.)

As economics has grown in scope and complexity, economists have begun to search for a single unifying concept around which to organize the massive collection of factual data and analytical tools which the student of economics is expected to master. Such attempts have failed so far, as in the brief vogue of consumer economics a few years ago, but the search by no means has been abandoned. Nevertheless, if success should be achieved it may not be the result of direct search but may well come through serendipity.

Certainly, the President's Materials Policy Commission did not set out to look for a unifying concept for economics, even though a study of its five-

volume report leads one to conclude that it may have taken a long step toward finding it. The Commission was charged with the responsibility of attempting to discover how large a supply of materials would be required by the American economy in the future and whether such materials would be forthcoming. In its attempt to answer these questions, the Commission undertook to examine the growth potential of the national economies of the "free world," and in particular to project the growth of the American economy to 1975. This emphasis on economic expansion is so consistently present and so well related to the discussion of various economic problems associated with the procurement of materials that it emerges as the central theme of the Report. Indeed, the handling of the growth concept is so skillful as to suggest the possibility of a general reorientation and integration of the diverse, specialized fields of economics around this single unifying concept. For the Commission's study of the materials problem has led it into virually every field of economic inquiry and has resulted in a document which contains something of interest and significance for many more economists than those whose primary interest is in the field of resourses utilization.

Price economics is represented in at least three forms. First, there is the insistence of the Commission that "nothing should be permitted to interfere with the principle that materials should be obtained at the least possible cost for equivalent values" (Vol. 1, p. 20), and its equal insistence on the fundamental importance of real cost as distinguished from money cost. Second, the Commission emphasizes the institutional character of the price-making process and examines those interferences with the processes of price determination which tend to hinder the development of a given resource, encourage its wasteful use, or discourage the introduction of substitute materials in the face of growing scarcity. Third, the Commission discusses the nature of cyclical price instability in the world markets for raw materials and relates these instabilities to the problems faced by the American economy in assuring itself of a steady and uninterrupted supply of essential materials, both for economic growth and for national security.

In the field of international trade the Commission examines the effects on materials acquisition of the multitudinous barriers to trade which exist in the world today. It discusses the possibilities of private and public investment in foreign countries as a means of securing for this country adequate supplies of needed or essential raw materials. In line with its emphasis on growth factors the Commission pays particular attention to the problems of economic expansion in the so-called underdeveloped areas of the world. In line with its necessary concern with problems of national security and defense, the Commission discusses the military strategy of raw materials supply; and in doing so divides the world into two groups—the "free, non-commuist world" and the communist group of national states. Both in its appraisal of the present world situation and in its recommendations for policy this Report offers substantial contributions to the economics of international trade.

There are, of course, sources of controversy in this part of the Report. If the projections of materials consumption for the United States and for the rest of

the "free world" are taken as forecasts they can only be challenged on technical grounds, but if they are interpreted as guides to policy a question immediately arises. For a policy based on these projections would seem to claim a lion's share of future output of many materials for the United States, while failing to allow for adequate aggregate expansion elsewhere. Some of the friendly nations are likely to describe this attitude as "economic imperialism," thinly disguised for the American reader by the Commission's justification of such policies by the principle of "enlightened self-interest."

On the other hand, the Commission does refuse to approve private or public policies of procuring our raw materials from the underdeveloped countries by the overt methods of colonial exploitation. Instead it cites with approval (and in Vol. V describes at some length) the policy of Venezuela which uses the revenues from the development of that country's petroleum resources to promote the education, social services and industrial progress of the Venezuelan people.

Turning to a third field of economics, it may be noted that the Commission attacks boldly and at length the delicate problems which arise in the general area of social control and economic planning. At the outset the Commission states that its members

> believe in private enterprise as the most efficacious way of performing industrial tasks in the United States. With this belief, a belief in the spur of the profit motive and what is called "the price system" obviously goes hand in hand (Vol. I, p. 3).

They further state that:

> We believe in a minimum of interference with these patterns of private enterprise. But to believe in a minimum of interference is not to believe that this minimum must be set at zero. Private enterprise itself has from time to time asked for helps, or restraints, or counterpoises from Government to keep the system working at its best; for this reason, among others, we have experienced for a long time a mixture of private and public influences on the functioning of our economy. The Commission sees no reason either to blink this fact or to decry it; as we see the future, the co-existence of great private and public strength is not only desirable but essential to our preservation (Vol. I, p. 3).

Probably there will be little disagreement with these propositions as a statement of principle. The rub comes when one attempts to define precisely what is included in, and excluded from, "a minimum interference with private enterprise." Throughout the Report there is a struggle with just such an attempt at definition. In Chapter 5 the Commission states the basis of its own policy recommendations as follows:

> The task of overcoming the materials problem is far greater than merely locating enough physical resources. The task is to overcome the multitudinous barriers described in the last chapter, but, more than that, to offer positive spurs and encouragements for developing and applying energy and technology to the materials field, for insuring a sufficient flow of capi-

tal into it, for guarding our security, and concerning ourselves at every point with insurance against rising costs (Vol. I, p. 17).

Having set its goals the Commission then recognizes the impossibility of achieving them without a program of economic planning, saying that:

> Such a set of accomplishments will never be achieved at random: only a consistent policy toward materials can hope to bring them about. Policy means here intelligently directed action toward consciously determined goals—as distinct from aimless drift and blind faith. It is not enough to solve the problem "eventually"; while the Nation waits it can encounter such a succession of individual shortages as to disrupt the cost pattern and defeat an "eventual" solution altogether (*Ibid.*).

The Commission then deals with the problems of materials supply assigned to it by the President by setting forth in Volume I some seventy-eight separate recommendations, most of which are specific and technical in nature and nearly all of which require government action. In addition, the Commission states its conclusions or beliefs on a larger number of other points which involve implications for policy decisions. These recommendations and suggestions for private and public policy are justified as being attempts to minimize interference with the least-cost principle as applied to the utilization of resources in the process of production and trade. Equally, the Commission's emphasis on real costs makes it possible to interpret the least-cost principle as dealing primarily with marginal social costs rather than with marginal private costs.

Many economists will agree with the Commission's philosophy, but professional and entrepreneurial opinions concerning the specific recommendations are likely to vary widely, partly for technical reasons, but also because of doubts whether the Commission's recommendations can be implemented without doing violence to the Commission's own belief "that the bulk of the task of insuring adequate future materials supply can best be carried out by private business under the competitive market structure, operating within broad policy outlines which it is the responsibility of the Government to provide" (Vol. I, p. 17). But whatever one's opinion on this issue it must be agreed that the Commission in dealing with the problem of the functions of advance planning and with the rôle of government in the contemporary economic affairs of the United States so directly and explicitly, has made a substantial contribution to present-day political economy. By the same token, it also has contributed to our understanding of the institutional aspects of economic growth.

The above discussion has dealt with what might be called the by-products of the Commission's primary task. Viewed as a statement of the problems of resources utilization for the next quarter-century the Report is worthy of very high commendation. It is almost the first government report on resources to emphasize the economic and administrative aspects of resources development rather than the physical. It is, in effect, an elegant and massive superstructure built on the foundations provided by Zimmermann and Dewhurst; although there are, quite naturally, many unfinished and unfurnished rooms in the edi-

fice.[1] The Report contains studies and reports on dozens of raw materials and examines in separate volumes the future outlook for key commodities (Vol. II), and for sources of energy (Vol. III). In Volume IV most of these commodities are re-examined in terms of the promise of future technological change. Volume V completes the Report with a series of supplementary studies selected from the large number prepared for the Commission. Although the Report gives consideration to virtually all materials, its discussion of problems of soil and water utilization is brief, and recommendations in these fields (except for industrial water supply) are left to other agencies.

No review could summarize or criticize adequately the many aspects of this Report. The Commission's recognition of the rôle of technology in our society, and of our failure to make the most of the promise of technology or even to train enough technologists is very important. Equally significant is the emphasis upon the need for further research in many directions—economic, administrative, and social as well as in the areas of the physical sciences. The presentation in a public document of the projections to 1975 of population, Gross National Product, and disposable income in the United States is even more exciting. Such projections provide the essential base for any realistic discussion of the problems involved, and of the policies required to assure the nation of adequate supplies of materials in the future. Nothing could bring home better to economists or to laymen the great promise of the future, or the reality of the problems to be solved if this promise is to be realized. For as the Commission itself observes:

> The five volumes of this Commission's Report are offered only as a beginning. The most important conclusion this Commission presents is thus that the job (of assuring materials supplies) must be carried on cooperatively by Government and private citizens, not periodically at wide-spread intervals, but day by day and year by year.

The ultimate influence of the President's Materials Policy Commission will depend upon how well the entrepreneurs of the American economy, both private and public, take heed of this conclusion.

MORRIS E. GARNSEY

University of Colorado

Housing Market Behavior In A Declining Area. By LEO GREBLER. Publication of the Institute For Urban Land Use and Housing Studies. (New York: Columbia University Press. 1952. Pp. vii, 265. $4.50.)

Dr. Grebler's book is the first of a projected series of studies in the behavior of urban housing markets undertaken by the Institute for Urban Land Use and Housing Studies of Columbia University. The Institute is under the able leadership of Dr. Ernest M. Fisher, universally recognized as a pioneer in the scientific investigation of these fields.

[1] *World Resources and Industries* (Rev. ed.), by Erich W. Zimmermann, (New York, Harper and Brothers, 1951). *America's Needs and Resources*, by J. Frederic Dewhurst and Assoc. (New York, The Twentieth Century Fund, 1947).

Through the intensive application of some known research methods and the development of some new and ingenious series, Grebler has succeeded in tracing the history of New York City's Lower East Side over a fifty-year period beginning in about 1900. His is not merely a general descriptive history, but much more importantly, an attempt to analyze market behavior in historical terms, to separate cause and effect, to identify and interpret the interaction of forces. Probably the most significant contribution of the book, however, is in fulfillment of one of its stated objectives, namely, to identify the materials and techniques required for the study of market behavior. It is to be hoped that another of the objectives will be realized: "to stimulate a number of local studies in various cities, which in the aggregate should make it possible to arrive at both generalizations and differentiations in respect to market behavior at the bottom end of the housing supply."

This is an exciting and stimulating book for students in the field, and these words are used advisedly, because they can but rarely be applied to research studies. While this reviewer could hardly be said to have read the book breathlessly he did have his interest held throughout, even while examining the charts and tables and the appendices, and even while reading Grebler's carefully expressed limitations upon a generalization of his findings based upon this single exploration. The absorption with which the book is read is not only a tribute to the material, its logical organization and unusually good writing but very significantly, to the fact that the reader, probably for the first time in his experience, is provided an almost complete set of data on real estate market phenomena and the interpretations of a capable analyst with which he can agree or disagree.

While recognizing the signal contribution of this book, both as an investigation and as a prototype for other studies, it was impossible to be free of the nagging feeling that the Lower East Side is an extremely atypical area and New York City is perhaps the most atypical of American cities. It was even somewhat difficult at first to accept this as a declining area in the general use of the term. This was an area which was originally built with relatively low grade housing many years before 1900. Even today, about 70 per cent of residential units, exclusive of public housing, are in "old-law tenements" (p. 121), which means units built before 1901. Furthermore, Grebler reports (p. 120) that during the last half of the nineteenth century and at the beginning of this century there is ample evidence that the quarters on the Lower East Side were below accepted standards, and (p. 6) that much of the area was considered by the community-at-large to be detrimental to health, safety, and welfare.

The case is made that this area in 1902 had an average rental not much below that for other areas in Manhattan. The normal implication of comparable rents is comparable quality. Grebler explains (p. 121) that the small differential in rents was probably due in part to "the almost compulsory factors which at that time caused immigrants, without knowledge of English and of ways of 'getting around' in a foreign country, to crowd into the area." The clear implication of this is that at the time the study begins in 1900, but for abnormal demand factors, there probably would have already been a wide

rental differential between the Lower East Side and the remainder of Manhattan.

It is evident that since 1900 the area has declined relative to the rest of Manhattan. The question arises: would the relative decline have been so great but for the fact that at the beginning of the period rents were held at a high level by unusual circumstances? Probably too much emphasis has been put upon this as a declining area. The purpose of the analysis would have been equally met—and the contribution certainly no less—if the study had been cast in the framework of market behavior over time in an area which was relatively low grade to begin with. In fact even less attention has been paid to the latter than to the decline of fashionable or upper-class neighborhoods.

After an excellent summary of present thought on the "filtering down" process, Grebler suggests that probably the best measure of "filtering" is the change in position of a dwelling unit or group of dwelling units in the distribution of rents and prices in the community as a whole. Unquestionably this measurement is a very useful one in analyses of market behavior. It does not appear, however, to measure the phenomenon that economists for a long while have described as the "filtering down" process, as defined for example, by Ratcliff (cited by Grebler, p. 56). It is difficult to see how the concept stated by Ratcliff, and generally accepted, can be subjected to measurement without data on both the incomes of occupants and the rentals or prices of the units. The reader is somewhat puzzled (pp. 61, 62) as to whether Grebler disagrees with the generally accepted concept of "filtration" or concludes pragmatically that the empirical data necessary to its testing cannot be assembled. He does not in any event make the case that his is a better or more rational measurement of the same phenomenon.

While much of this review has been used for raising questions about Grebler's book, to have said all the good things about it would have taken infinitely more space. This book is a major contribution and marks a significant advance in the understanding of both the data required and the concepts involved in analyses of market behavior. It is highly important for Grebler, and the Columbia Institute, to push on.

LAWRENCE N. BLOOMBERG

Washington, D.C.

Labor

The Impact of the Union. Edited by DAVID MCCORD WRIGHT. (New York: Harcourt, Brace. 1951. Pp. ix, 405. $4.00; text edition, $3.00.)

This volume seems to have developed from a feeling that—to paraphrase a famous remark about generals—labor economics is too important to be left to the labor economists. This reviewer, who tries to be friends with theorists, labor economists, and union officials, can only feel that a dirty trick was played in asking him to write this review; and he hopes that he may have a few friends left after the review has been written and read.

It is undoubtedly true that wage theory cannot be handled most effectively

as a specialty, but must be treated in context as part of a larger apparatus of analysis. A revival of interest in wages by general economic theorists is long overdue and much to be desired. This volume, the most provocative group of essays about wages which has appeared in recent years, suggests that such a revival may be in the making.

The volume includes papers by Professors Clark, Haberler, Knight, Boulding, Chamberlin, Friedman, Samuelson, and Wright. The papers were written for and discussed at a conference sponsored by the American University. Most of the round-table discussion of the papers by the eight participants has been included in the volume, forming about one-third of the total text. The discussion is interesting and helps to bring out the distinctive flavor of each man's thought.

Despite the effort at integration through a conference, the papers do not hang together very well. All make some mention of wages, but this provides only a tenuous link. Knight's paper is a general exposition of the rationale of a competitive economy, with rather brief reference to wage-setting toward the end of the paper. Boulding restates the macro-economic theory of distribution first presented in his *Reconstruction of Economics*, the tenor of which is that events in the labor market may have relatively little to do with aggregate distributive shares. Wright's first paper (there are two in the volume) discusses the dilemma of progress versus security along lines reasonably familiar from his earlier writings. Samuelson reviews and criticizes different types of theorizing about wages which have been important over the past century or so. The merit of the book consists in the brilliance of some of the papers as discrete entities, rather than in any unified reconstruction of wage theory. An integrated treatment is not achieved, and doubtless could not have been achieved in this sort of symposium.

It was probably overambitious to title this volume *The Impact of the Union*. One may well ask, "*Which* impact?" Leaving aside all noneconomic effects, there are many economic consequences of collective bargaining which do not operate via the wage structure—effects on the size and composition of the labor force, allocation of jobs, labor mobility and turnover, worker effort and efficiency, production methods, plant location, marketing and competitive tactics. While the quantitative importance of these effects is difficult to judge, it would not be surprising if wage effects turned out to be a minor part of the total. Nor is it very useful to say that any cost effect of unionism can be assimilated for theoretical purposes to a wage change. This is to work at a level of generality which veils many of the most significant problems. While the contributors to the volume doubtless realize all this, the dominant emphasis on wages conveys an impression that other economic effects are regarded as less important.

Again, what significance can be attached to "*the* union"? This implies a homogeneity in union policies and tactics which does not exist. Even within the United States, national union policies show significant variation at a given time and change over the course of time. Several of the papers imply that everybody knows what a union is and what it does, that theoretical reasoning

can safely start from simple premises which are matters of common knowledge. This is certainly a dubious position, which may lead to waste of theoretical ingenuity in analyzing cases quite remote from reality.

Theorists might reply that labor economists have not yet provided usable generalizations about union behavior and that, in the absence of empirically grounded assumptions, they are forced to proceed as best they can on an intuitive basis. While there is something to this, some of the papers in this volume fail to draw even on the limited knowledge of union behavior which we do have. It would certainly have been a good idea to include a student of trade unionism in the conference. The categories "theorist" and "labor economist," while they do not coincide as closely as one might wish, are not so mutually exclusive as the list of contributors to this volume would suggest.

The traditional (and doubtless healthy) tendency of economists to view with alarm is evident throughout the volume, but something new has been added. Earlier economic critiques of trade unionism emphasized mainly its effects on relative wage rates and on resource allocation. The main fear expressed in this volume, however, is that the *general* level of money wage rates may rise too rapidly as a result of union activities. Cost-induced inflation may produce a chronic tendency toward rising prices accompanied by unemployment, or frequent declines in employment if the monetary authorities attempt to hold prices in check. The mechanisms involved are suggested in Haberler's skilful and lucid analysis.

Friedman suggests that our present fears on this score may be exaggerated, and that the effect of unionism both on wage structure and on the money wage level may be overstated in popular discussion. This is an interesting paper, the only one in the volume which makes frequent reference to empirical evidence. The reviewer is inclined to share Friedman's doubts about the importance of union wage effects in the postwar period. Looking back twenty years from now, it may turn out that present theories of chronic inflation are as lopsided as were our theories of perpetual depression in the late 'thirties. Even economists are too largely creatures of their times.

If the problem does exist, something should presumably be done about it. There is something like a consensus among the authors that not much reliance can be placed on monetary-fiscal restraints on inflation, because of the unwillingness of a democratic government to risk a serious recession. Beyond this, however, there is little in the way of positive prescription. Several contributors appear to hope that union bargaining power can be reduced by restricting unions to a one-company basis, by curbing certain types of union tactic, and so on. These proposals are not pressed very seriously, however, and one suspects that even their proponents doubt their workability. Clark, in a typically statesmanlike paper, explores the possibility of bringing about changes in union behavior, of subordinating short-run conceptions of the union's interest to longer-run definitions of self-interest. The fact that none of the authors appears highly optimistic about this or any other approach simply reflects the general state of mind of economists on this matter.

This is certainly a book which deserves to be read by economists generally,

not simply by students of labor. It should have considerable usefulness for graduate students and for the more capable undergraduates. It is bound to provoke dissent, quite possibly indignant dissent, in some quarters. If this leads to more and better work on wage questions, however, the authors will not have labored in vain.

LLOYD G. REYNOLDS

Yale University

Population; Social Welfare and Living Standards

Grundriss der Bevölkerungswissenschaft (Demographie). By RODERICH V. UNGERN-STERNBERG and HERMANN SCHUBNELL. (Stuttgart: Piscator-Verlag. 1950. Pp. viii, 602. DM 42.—.)

This book by two experienced German demographers embodies an effort to present a systematic analysis of population questions. It commences with the study of the main features by which a population may be characterized and measured, *e.g.*, households, size of town or city, trade and social position, occupation, age and sex, health and educational attainments. With this groundwork laid, population is studied in its process of renewal through natural change arising out of births and deaths. The subject is covered by establishing some pivotal position which is traced in its various admixtures and correlations. Systematically canvassed in this fashion are available materials on the family and marriage, births and deaths. These materials are then recapitulated in summary of resultant population movements and problems of their measurement and forecasting. By restricting canvass and compressing treatment, rapid disposition is made of the three remaining major topics: migration and processes of spatial population movement, impact of war on population, and population policy and theory.

The work achieves an externally efficient mode of organization of its materials but at the cost of much repetition of subject matter and superficiality of treatment. Repetition is involved since the study of phenomena in their statistically emergent forms (thus birth rate, family size, child mortality, population growth) involves segregated treatment not of different aspects of a process but only of different statistical expressions of it. The analysis remains at the surface level for the most part. Thus in the treatment of population as an aggregate only the most superficial review of statistical findings about population is given. No effort is made to differentiate social systems according to the way in which population is handled. And since the authors regard all aggregative phenomena concerning population as suitable for inclusion within their discipline, the inner connection of these phenomena are lost while attention is concentrated on classification or registration of their outward features.

Even as a handbook the work is deficient. Though most of its bulk is taken up with presenting data on a world basis, both the scope and adequacy of the reporting are limited. Undue prominence is given to recent German developments; little systematic attention is given to population phenomena in non-

European societies; and little use is made—both on the analytic and informational side—of the immense wealth of pertinent monographs and literature outside Germany on the subject. Thus the chapter on internal population movement or mobility fails to take account of the American literature on ecological research and Sorokin's basic monograph on social mobility. Very little in French population literature is cited and of the American literature the authors show acquaintance chiefly with Thompson's text, *Population Problems*. As usual little attention is given to Malthus' positive analysis in the field of demography though the customary homage to his name is paid in the skimpy chapter on the history of population policy. Thus the chapter of the book dealing with the impact of war on population (confined, by the way, chiefly to the statistical registration of the effects of World War I on leading belligerent countries) could have taken as a point of departure Malthus' perceptive analysis directed to the failure of the Napoleonic wars to affect more prominently than it did French population growth (see Bk. II, Ch. 6, of Malthus' *Essay,* later editions).

Though the work is unsuitable as a handbook, it occasionally brings to light some interesting materials. Thus on many relatively obscure questions (such as marriage rates by calendar months, illegitimacy, twins, institutional births, etc.) information not otherwise readily available might be found in this work. American students will be particularly interested in the presentation of German census reports on family size of completed marriages published in Germany during the war and released in a form not readily available in most American libraries. These reports provide "cohort" information on family size in a detail matched by few countries (see pp. 266-77). Comparison of the French and German data on family size in the 'thirties brings to light that differences between countries as to average family size arise from different assortments of family types—the relative weight of the sterile couple, the short 2-3 children family and the long over-4 family—and not from any absolute differences in behavior or modal performance. It is highly likely that the whole modern reduction of the birth rate below reproductive levels reflects the growth in sterile marriages, a slight increase in the rôle of one and two-child families, and the marked diminution in the number of families rearing to adulthood four or more children. But instead of concentrating on this crucial outcome and drawing on more complex and remote levels of experience and causation to explain it, this work like so much of the recent literature in the field obscures fundamental issues by failing to give them the central importance they deserve. It is also worth noting that the discussion of German data on family size does not make a comparative analysis of recent British and American data on the same subject.

Manuel Gottlieb

University of Kansas City

TITLES OF NEW BOOKS

Economic Theory; General Economics

BAUDIN, L. *Manuel d'économique politique.* Vol. 1. (Paris: Lib. Générale de Droit et de Jurisprudence. 1953. Pp. 624.)

This is the seventh edition of this text in principles of economics. The first volume, after a short history of economic thought and after providing definitions of economics, needs, goods, utility, etc., is devoted to the subjects of population, the price mechanism, production, money and credit.

BOULDING, K. E. *The organizational revolution—a study in the ethics of economic organization.* With commentary by Reinhold Niebuhr. Ser. on Ethics and Econ. Life by a study committee of the Federal Council of Churches. (New York: Harper & Bros. 1953. Pp. xxxiv, 286. $3.50.)

BRESCIANI-TURRONI, C. *Corso di economia politica.* Vol. 1, *Teoria generale dei fatti economici.* Rev. ed. (Milan: D. A. Giuffrè. 1953. Pp. 434.)

The second edition of a book originally published in 1949. Additional examples of the quantitative analysis of economic phenomena are provided; and a summary account of the author's investigations on the subject of Pareto's Law, first published some decades ago, is also included.

CHARNES, A., COOPER, W. W., and HENDERSON, A. *An introduction to linear programming.* (New York: John Wiley & Sons. 1953. Pp. ix, 74. $2.50.)

Part 1 is an economic introduction to linear programming and contains sections designed to indicate the nature of linear programming for the nonspecialist, as well as sections for the person interested in concrete applications of the technique. The final sections of this part deal with more advanced materials. Part 2 is devoted to the mathematical theory of linear programming, but is "designed so that a minimum of beginning background mathematics is required."

DATTA, B. *The economics of industrialisation—a study of the basic problems of an underdeveloped economy.* (Calcutta: The World Press, Ltd. 1952. Pp. vi, 291. Rs. 12.8; 21 *s.*)

"The present study examines the impact of the employment and output approach on the economic policy of underdeveloped countries and analyses the cyclical, seasonal and structural problems of underemployment vis-à-vis the prospects and difficulties of industrialisation. Special emphasis has been put on the significance of 'disguised unemployment' as relevant to backward economies, the relative importance of the determinants of employment and income, the relation between the occupation-pattern and resource-utilisation, the problem of the 'surplus agricultural population' and the problem of measuring this surplus under changing resource-utilisation and changing terms of trade."

DEHEM, R. *L'efficacité sociale du système économique.* (Louvain: Institut de Recherches Econ. et Soc. 1952. Pp. 184. $3.)

A review of recent and contemporary welfare economics with primary emphasis upon the theory concerned with optimum use of resources. Because the author finds a contemporary dichotomy between two theories of social welfare—that concerned with optimum use of resources, and the theory of employment—he also includes a treatment of the latter and considers the possibility of an integration of these two aspects of welfare economics in the broader sense.

EUCHEN, W. *Grundsatze der wirtschaftspolitik.* (Bern: Francke. Tubingen: J. C. B. Mohr. 1952. Pp. lx, 396.)

EUGSTER, C. *Thorsten Bunde Veblen, 1857-1929. Darstellung und Deutung amerikanischen institutionellen Denkens aus seinem Werk heraus.* (Zurich: Europa Verlag. 1952. Pp. 116. 116 *s.*, Kart. fr. 6.75.)

FOSSATI, E. *Frammenti di teoria dinamica.* (Trieste: Università di Trieste. 1952. Pp. x, 135.)

FOURASTIÉ, J. *La productivité.* No. 557 of the collection Que sais-je? (Paris: Presses Univ. de France. 1952. Pp. 118. 150 fr.)

The approach of this little book is similar to that of the author's major work cited below.

———. *Le grand espoir du XXe siècle—progrès technique, progrès économique, progrès social.* 3d ed. (Paris: Presses Univ. de France. 1952. Pp. xxviii, 245. 320 fr.)

The object of the author is to show the economic consequences of technical progress. The approach is a very broad one. There are chapters on migration, the evolution of capitalism, unemployment (technological and cyclical), international trade and world equilibrium, and the level of living.

GEMMELL, J. and BALSLEY, H. L. *Principles of economics.* (New York: D. C. Heath. 1953. Pp. xiii, 589. $4.75.)

GRAMPP, W. D. and WEILER, E. T., editors. *Economic policy—readings in political economy.* (Homewood, Ill.: Richard D. Irwin. 1953. Pp. xiii, 393. $3.75.)

A collection of readings on four main areas of economic policy: stability, the control of monopoly power, the distribution of income, and international economic relations. "The book is meant for courses in principles of economics, and it also can be used in courses in government and business, economic problems, and economic planning." The selection of materials appears excellent.

HARROD, R. F. *Economic essays.* (New York: Harcourt, Brace & Co. 1953. Pp. xiii, 301. $4.50.)

HENDERSON, J. S. *Production and consumption—a mathematical reformulation.* Univ. of Alabama stud. no. 7. (University, Ala.: Univ. of Alabama Press. 1952. Pp. 83. $2.)

HIGGS, H. *The Physiocrats—six lectures on the French* Économistes *of the 18th Century.* Reprint of the English ed. of 1897. (New York: The Langland Press. 1952. Pp. x, 158. $3.25.)

HUTCHISON, T. W. *A review of economic doctrines—1870-1929.* (Oxford, Eng.: Clarendon Press. 1953. Pp. xiv, 456. $6.)

LAGLER, E. and MESSNER, J., editors. *Wirtschaftliche Entwicklung und soziale Ordnung.* (Vienna: Verlag Herold. 1952. Pp. 456. $6.)

MARCHAL, A. *La pensée économique en France depuis 1945.* (Paris: Presses Univ. de France. 1953. Pp. vii, 240. 700 fr.)

MACHLUP, F. *The economics of sellers' competition: model analysis of sellers' conduct.* (Baltimore: Johns Hopkins Press. 1952. Pp. xx, 582. $6.50.)

PIGOU, A. C. *Essays in economics.* (New York: St. Martin's Press. 1952. Pp. vii, 241. $3.)

A collection of economic papers, mostly nontechnical in character, that have (with three exceptions) appeared elsewhere since 1939. The three new papers are: "The 'Value' of Different Kinds of Transfer Payment," a short review of *Poverty and the Welfare State*, by Rowntree and Labers; "The Inflationary Gap," written in 1951; and "The Gold and Dollar Reserve," written in the spring of 1952. The volume also includes "Wage Statistics and Wage Policy," the University of London Stamp Memorial lecture, 1949 (published earlier by the Oxford University Press); "One Way of Looking at Economics," printed in *Economica*, January 1951; "Employment Policy," a review of *Full Employment in a Free Society*, by Sir William Beveridge; and numerous shorter essays.

POOL, A. G. *Economists and social policy.* (Leicester: Univ. College, Leicester. 1952. Pp. iii, 20. 1 s.)

RÖPER, B. *Die Konkurrenz und ihre Fehlentwicklungen—Untersuchungen über Störungen der Marktwirtschaft.* (Berlin: Duncker & Humblot. 1952. Pp. 243. DM 14, 60.)

STARK, W. *Jeremy Bentham's economic writings.* Vol. 1. (New York: Burt Franklin. 1952. Pp. 412.)

TINBERGEN, J. *On the theory of economic policy.* Contributions to Economic Analysis, I., edited by J. Tinbergen, P. J. Verdoorn and H. J. Witteveen. (Amsterdam: North-Holland Pub. Co. 1952. Pp. 80. $1.80.)

Ward, A. D., editor. *Goals of economic life.* Ser. on Ethics and Econ. Life by a study committee of the Federal Council of Churches. (New York: Harper. 1952. Pp. x, 470. $4.)

Williams, J. H. *Economic stability in a changing world: essays in economic theory and policy.* (New York: Oxford Univ. Press. 1953. Pp. 284. $5.)

John R. Commons, teacher, economist and administrator. Addresses delivered on Oct. 10, 1950, in commemoration of his achievements. (Madison: State Historical Soc. of Wisconsin. 1952. Pp. vii, 23.)

Problèmes économiques contemporains. Colloquia de la chaire Francqui 1951-52. (Liège: Faculté de Droit de l'Université de Liège. 1953. Pp. viii, 150.)

Studi Keynesiani. Prepared by the Instituto di Economia e Finanza della Facoltà Giuridica di Roma. (Milan: A Giuffrè. 1953. Pp. xi, 385. L. 1500.)

Economic History; National Economies; Economic Development

Allbaugh, L. G. *Crete, a case study of an underdeveloped area.* Report of a study carried out by the Rockefeller Foundation. (Princeton: Princeton Univ. Press. 1953. Pp. xx, 572. $7.50.)

Bog, I. *Die Bäuerliche Wirtschaft im Zeitalter des dreissigjährigen Krieges.* (Coburg: Veste Verlag. 1952. Pp. xiii, 180.)

Call, T. C. *The Mexican venture—from political to industrial revolution in Mexico.* (New York: Oxford Univ. Press. 1953. Pp. xii, 273. $4.50.

The book is concerned with social, economic, and political aspects of recent developments in Mexico. "It is an effort to appraise, in the light of recorded facts and personal observation, all major phases of modern Mexican life, to view what the Revolution has accomplished toward forging a nation, and to consider what the future may hold for the people."

Clough, S. B. *The American way—the economic basis of our civilization.* (New York: Thomas Y. Crowell. 1953. Pp. viii, 246. $4.)

Cooke, H. V. *Challenge and response in the Middle East—the quest for prosperity, 1919-1951.* (New York: Harper. 1952. Pp. 366.)

Czarnomski, F. B. *Can Russia survive? An examination of the facts and figures of Soviet reality.* (New York: Philosophical Library. 1953. Pp. 128. $2.75.)

Eaton, J. *Economics of peace and war—an analysis of Britain's economic problems.* (New York: Internat. Publishers. 1953. Pp. 112. $1.50.)

A very pessimistic view of the future of the British economy is presented. "As part of the capitalist world Britain is dragged down with the capitalist world as a whole. But on top of this British imperialism faces its own crisis." The British Labour Party's program was a retreat from socialism. The Marshall Plan was designed "to co-ordinate ('integrate') Europe in such a manner as would, the Americans hoped, bring it under the economic and political control of the U.S.A. and prepare the way for the aggressive military plans that took shape in N.A.T.O." And so on.

Friedlaender, H. E. and Oser, J. *Economic history of modern Europe.* (New York: Prentice-Hall. 1953. Pp. xxiv, 611. $6.)

Gayer, A. D., Rostow, W. W., Schwartz, A. J. *The growth and fluctuation of the British economy 1780-1850.* 2 vols. (New York: Oxford Univ. Press. 1953. Pp. xix, 528; xii, 1028. Vols. I & II, $17.)

Gutierrez, G. *El desarrollo économico de Cuba.* Estud. e Investigaciones Econ. ser. no. 12. (Havana: Junta Nacional de Economia. 1952. Pp. xxi, 257.)

Hughes, E. *North country life in the eighteenth century: The north east 1700-1750.* (New York: Oxford Univ. Press. 1952. Pp. xxi, 435. $6.)

Based upon a collection of 18th century manuscripts discovered in 1940—the letters, account books, and other papers of the lords of the manors of Gateshead and Whickham, "once the richest coal-bearing manors in this country" (From the preface).

JENNINGS, W. W. *Twenty giants of American business: biographical sketches in economic history.* (New York: Exposition Press. 1953. Pp. 480. $5.)

JONES, N. S. C. *The pattern of a dependent economy: a study of the national income of British Honduras.* (New York: Cambridge Univ. Press. 1953. Pp. xv, 162. $4.)

National income tables for British Honduras for 1946 have been prepared, in spite of the difficulty in finding adequate data. The author has had two aims: "first, to show that a reasonably accurate national income study, in broad outline, can be made without overloading it with detail or spending an inordinate time in detailed research; and, secondly, to indicate some of the problems peculiar to a dependent economy by an examination in some detail of one."

KUCZYNSKI, J. *Studien zur Geschichte des deutschen Imperialismus.* I, *Monopole und Unternehmernerbände.* (Berlin: Dietz. 1952. Pp. v, 398.)

LANE, F. C., ed., assisted by RIERMERSMA, J. C. *Enterprise and secular change—readings in economic history.* (Homewood, Ill.: Richard D. Irwin, for the Am. Econ. Assoc. and the Econ. Hist. Assoc. 1953. Pp. xi, 556.)

MAYER, A. C. *Land and society in Malabar.* Issued under auspices of Internat. Secretariat Inst. of Pacific Relations. (New York: Oxford Univ. Press. 1952. Pp. vii, 158. $3.)

"This book is an analysis of the social system of one part of India in relation to its most important base, the system of agriculture."

MULHAUPT, L. *Strukturwandlungen und nachkriegsprobleme dere wirtschaft Schwedens.* (Kiel: Institut fur Weltwirtschaft, Universität Kiel. 1952. Pp. iii, 187.)

MÜLLER-OHLSEN, L. *Strukturwandlungen und Nachkriegsprobleme der Wirtschaft Frankreichs.* Kieler stud. 22. (Kiel: Institut fur Weltwirtschaft, Universität Kiel. 1952. Pp. vii, 205. Dm. 16.-.)

SEERS, D. and ROSS, C. R. *Report on financial and physical problems of development in the Gold Coast.* (Accra, Gold Coast: Govt. Printing Office. 1952. Pp. iii, 65. 5 *s.*)

SUMMERS, F. P. *William L. Wilson and tariff reform.* (New Brunswick: Rutgers Univ. Press. 1953. Pp. xi, 288. $5.)

The biography of a political leader of the Cleveland era who championed a low tariff.

TWITCHELL, K. S. *Saudi Arabia—with an account of the development of its natural resources.* 2nd ed. (Princeton: Princeton Univ. Press. 1953. Pp. xxi, 231. $5.)

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NOTES

A nominating committee consisting of M. M. Bober, Edward S. Shaw, George W. Stocking, George W. Taylor, Willard L. Thorp and John H. Williams, chairman, has submitted the following slate of nominees for 1954 officers of the American Economic Association:

President: Simon S. Kuznets, University of Pennsylvania

Vice President:
Roy Blough, Director, Department of Economic Affairs, United Nations
William J. Fellner, Yale University
Jacob Marschak, University of Chicago
Arthur Smithies, Harvard University

Executive Committee:
Moses Abramovitz, Stanford University
Evsey D. Domar, John Hopkins
Norman S. Buchanan, University of California
George J. Stigler, Columbia University

Representative of Social Science Research Council:
D. Gale Johnson, University of Chicago

The annual meeting of the Association will be held at the Hotel Statler, Washington, D.C., December 28-30, 1953. The meeting is to be held jointly with allied social science associations. Thus far the Econometric Society, the American Finance Association, the Economic History Association, the American Statistical Association and the Industrial Relations Research Association have indicated their intention of meeting at the same time in Washington.

Deaths

Herbert F. Fraser, professor of economics at Swarthmore College, died February 9, 1952.

Donald J. Hay, instructor in marketing at the University of Illinois, died November 1, 1952.

Frederick E. Lee, of the department of economics, University of Illinois, died September, 1952.

Earl A. Saliers, of Louisiana State University, died December 23, 1953.

Retirements

Clyde B. Aitchison, after serving almost thirty-five years as an Interstate Commerce Commissioner.

John D. Black, as Henry Lee Professor of Economics, effective July, 1953.

Theodore T. Bullock, professor of economics and business law, College of Business Administration, University of Nebraska, February, 1953.

Harry W. Cordell, associate professor of marketing, the Ohio State University, June, 1953.

William H. Spencer, Hobart W. Williams Distinguished Service Professor of Government and Business, effective September, 1953.

Thomas L. Kibler, professor of transportation at the Ohio State University, June, 1953.

Appointments and Resignations

Robert Agnew has been appointed assistant professor of industry in the School of Business Administration, University of Pittsburgh.

A. Anastassiades has been appointed instructor in economics in the School of Business Administration, University of Pittsburgh.

Edward A. Anderson has been appointed instructor in marketing in the Wharton School, University of Pennsylvania.

Arthur G. Auble, of Northwestern University, has been appointed associate professor of business statistics at Claremont Men's College.

Edwin H. Baldridge has been promoted to assistant professor of accounting in the School of Business Administration of the University of Pittsburgh.

Claude D. Baldwin, formerly in the Bureau of the Budget, is now economist in the Office of the Secretary of Defense.

William J. Baumol, of Princeton University, is visiting professor of economics at the University of California, Berkeley, in the spring term.

Edward G. Bennion, director of the General Economic Division of the Standard Oil Company of New Jersey, will be visiting professor of economics at the Massachusetts Institute of Technology in 1953-54.

William Beranek has been appointed lecturer in finance in the School of Business Administration, University of California, Los Angeles.

Hilary R. Beth has been appointed instructor in management in the College of Business Administration, Tulane University.

R. A. Blackwood has been appointed instructor in business administration at Louisiana State University.

Arthur I. Bloomfield, senior economist of the Federal Reserve Bank of New York, has returned from the Associated States of Indochina where he served as economist for a Mutual Security Agency mission.

Robert W. Bradbury is resuming his duties at the University of Florida after a year in foreign service with the Department of State.

Robert B. Buchele has been appointed assistant professor of personnel management and industrial relations at the University of California, Los Angeles.

Arthur F. Burns, of Columbia University, has been appointed to the Council of Economic Advisers to the President.

John M. Clark was appointed special lecturer in the department of economics, Columbia University, upon becoming professor emeritus in February 1953.

Edward Coen, of the London School of Economics, was visiting lecturer at the University of Minnesota in the spring quarter.

Gerhard Colm, chief economist of the National Planning Association, has been in Korea consulting on matters relating to rehabilitation and in India consulting with Indian leaders concerned with national economic planning.

Edward J. Cook has been promoted to assistant professor in the School of Business, Fordham University.

C. H. Donovan has been appointed head of the department of economics in the College of Business Administration of the University of Florida.

Frederick Downs, of the University of Wisconsin, has accepted an appointment as research associate with the University of Kentucky Bureau of Business Research.

James S. Duesenberry has been promoted to associate professor of economics at Harvard University.

James S. Earley, of the University of Wisconsin, has been granted a leave in 1953-54 to accept a Carnegie Foundation grant in connection with the Directed Studies Program at Yale University.

Joseph B. Eisenberg has been appointed instructor in industry in the Wharton School, University of Pennsylvania.

Philip Elkin has been appointed instructor in insurance in the Wharton School, University of Pennsylvania.

Solomon Fabricant has been appointed deputy director of research at the National Bureau of Economic Research.

Gerald J. Feldman has been appointed instructor in industry in the Wharton School, University of Pennsylvania.

George N. Francis has been promoted from assistant professor to associate professor of accounting at Los Angeles State College of Applied Arts and Sciences.

Irwin Friend has been appointed lecturer in finance at the Wharton School, University of Pennsylvania.

Wytze Gorter, of the University of California, Los Angeles, has been awarded a Carnegie

Research Fellowship by the Council on Foreign Relations, at whose New York headquarters he will conduct research in 1953-54.

Daniel H. Gray has been appointed assistant professor of economics at Tufts College for the year 1953-54.

Walter Heim has been appointed assistant professor of accounting in the School of Business Administration, University of Pittsburgh.

Richard M. Heins has been appointed lecturer in insurance in the School of Business Administration, University of California, Los Angeles.

Charles H. Hession has been promoted from assistant professor to associate professor of economics at Brooklyn College.

Werner Z. Hirsch, of the University of California at Berkeley, has been appointed assistant professor of economics at Washington University, St. Louis.

Thomas P. Hogan has been appointed instructor in economics in the School of Business Administration, University of Pittsburgh.

Carl Iversen, of the University of Copenhagen, has been appointed visiting professor of political economy at the Johns Hopkins University for the February 1954 term.

C. Hayden Jamison has been appointed chairman of the department of economics and finance at Beloit College in the absence of Dr. Lewis Severson.

Eugene E. Jennings has been appointed assistant professor of industry at the Wharton School, University of Pennsylvania.

John E. Jeuck has been promoted to professor of marketing and succeeds Garfield V. Cox as dean of the School of Business, University of Chicago.

Michael J. Jucius, now at the University of Turin, Italy, will resume his duties as professor of personnel at the Ohio State University in the autumn quarter.

K. William Kapp has been promoted from associate professor to professor of economics at Brooklyn College.

Charles P. Kindleberger, of the Massachusetts Institute of Technology, will be on leave in 1953-54 to conduct research, using the facilities of the Economic Commission for Europe at Geneva, Switzerland, on a grant from the Merrill Foundation for Advancement of Financial Knowledge.

Walter Kirk has been appointed assistant professor of finance in the School of Business Administration, University of Pittsburgh.

John P. Lewis has been appointed associate professor of business administration in the School of Business, Indiana University.

E. E. Liebhafsky has accepted an appointment as assistant professor of economics at Pennsylvania State College.

Bernard S. Logan has been appointed assistant professor of economics in the School of Business Administration, University of Pittsburgh.

Clarence D. Long, of Johns Hopkins University, has been visiting professor of economics at Columbia University for the spring term.

David L. Lutin, formerly research associate at Massachusetts Institute of Technology, is now an economist in the Office of the Administrator, Housing and Home Finance Agency, Washington, D.C.

Wilfred Malenbaum has joined the staff of the International Studies group at Massachusetts Institute of Technology.

C. Arnold Matthews has returned to the University of Florida after having served with the Armed Forces since 1951.

Dan M. McGill has been appointed associate professor of insurance in the Wharton School, University of Pennsylvania.

Edmund A. Mennis has resumed his duties as security analyst on the research staff of the Wellington Fund in Philadelphia after service in the Military Air Transport Service.

Hermann Meyer-Lindenberg, of Bogota, Colombia, has been appointed visiting professor of economics for the spring term at the University of California, Berkeley.

Raymond F. Mikesell will be on leave from the University of Virginia August to December this year to serve as visiting professor at the National War College.

Ralph A. Nittinger has been appointed instructor in insurance at the Wharton School, University of Pennsylvania.

Thaddeus J. Obal has resigned from the Congressional Joint Committee on the Economic Report to accept a position as economic analyst in the Ford Motor Company.

John F. Orchard has been appointed acting dean of the Graduate School of Business, Columbia University.

Raymond R. Orie has been appointed instructor in accounting in the School of Business Administration, University of Pittsburgh.

Grant M. Osborn has been appointed instructor in insurance in the Wharton School, University of Pennsylvania.

A. R. Oxenfeldt has resigned as associate professor of economics at the City College.

Irving Pfeffer has been appointed instructor in insurance at the Wharton School, University of Pennsylvania.

Karl Polanyi has terminated his services with Columbia University as visiting professor of economics as of February 1953.

Hoyt Price is now second secretary in the American Embassy at Saigon.

John M. Rathmell has been appointed lecturer in marketing at the Wharton School, University of Pennsylvania.

Richard Reed has been appointed instructor in economics at Wheaton College.

George F. Rohrlich has been appointed chief of the Division of Actuarial and Financial Services, Unemployment Insurance Service, Bureau of Employment Security, Department of Labor.

Sam Rosen has been appointed assistant professor of economics at the University of Delaware.

Hilda Rosenbloom has returned to Wellesley College as assistant professor of economics.

James H. Rossell has been promoted to assistant professor of accounting in the School of Business Administration, University of Pittsburgh.

Simon Rottenberg has been appointed visiting assistant professor and research associate at the Research Center in Economic Development and Cultural Change and Industrial Relations Center, University of Chicago.

Eugene Rotwein has been granted a leave of absence from the University of Wisconsin in 1953-54 to accept a Carnegie Foundation grant in connection with the Contemporary Civilization Program at Columbia University.

Francisco R. Saenz, formerly attaché to the Economic Division of the Mexican Embassy in Washington, is now with the foreign department of the Manufacturers Trust Company, New York, N.Y.

David J. Saposs, who has been special adviser to the director of the European Labor Division of the Mutual Security Agency in Paris, has returned to his post as special assistant to the Commissioner of Labor Statistics, Department of Labor.

Richard Scheuch has been promoted to assistant professor of economics at Trinity College.

Morton J. Schussheim, economist with the Cleveland City Planning Commission, has been visiting lecturer in urban land economics at Western Reserve University in the spring term.

Frederick A. Schwarz has been appointed assistant professor of accounting in the School of Business Administration, University of Pittsburgh.

Carlo Sciffo has been appointed instructor in economics in the School of Business Administration, University of Pittsburgh.

Robert W. Semenow has been promoted to associate professor of real estate in the School of Business Administration, University of Pittsburgh.

Irving A. Sirken, formerly of Williams College, has accepted a position as chief of Industrial Analysis and Program Evaluation, Economic Development Administration, San Juan, Puerto Rico.

George R. Taylor, of Amherst College, has been visiting professor of economics at Columbia University in the spring term.

Reed Tripp, of the University of Wisconsin, has accepted a position with the University of Kentucky Bureau of Business Research.

Randall W. Tucker has been promoted to assistant professor of economics at Trinity College.

Ralph Turvey, of the London School of Economics and Political Science, has been appointed visiting lecturer at the Johns Hopkins University for the October 1953 term.

Lloyd Ulman, of the University of Minnesota, has been awarded a three-year faculty fellowship by the Social Science Research Council.

Arthur R. Upgren has been appointed dean of the Amos Tuck School of Business Administration, Dartmouth College.

Daniel C. Vandermeulen will return to Claremont Men's College in September as associate professor of economics.

Royal S. Van de Woestyne has assumed the dual position of associate dean and dean of students of the School of Business, University of Chicago.

Charles W. Voris has resigned from Washington State College to join the staff of Los Angeles State College of Applied Arts and Sciences, as assistant professor of industrial management.

Louis A. Werbaneth has been appointed instructor in accounting in the School of Business Administration, University of Pittsburgh.

Robert H. Wessel has been promoted to assistant professor of economics at the University of Cincinnati.

Frank N. Willets has been appointed assistant professor of accounting in the School of Business Administration, University of Pittsburgh.

Max A. Woodbury has been appointed associate professor of statistics in the Wharton School, University of Pennsylvania.

William Woodruff, of Nottingham University, Nottingham, England, has accepted an appointment as associate professor of economics at the University of Illinois, effective September 1953.

H. Edward Wrapp has been appointed associate director of the Executive Program in the School of Business, University of Chicago.

Bertram W. Zumeta has been appointed instructor in statistics in the Wharton School, University of Pennsylvania.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

Research in economic security: National organization located in Chicago seeks person with wide research background and knowledge of survey work, preferably with some experience in conducting meetings and educational programs. P158

Economic analyst: Well-known national organization doing long-range economic study work on national business problems desires trained research economist with writing ability. Location, Chicago. Give personal qualifications, including education, job experience, and salary record. Replies will be held in strict confidence. P159

Economists Available for Positions

Personnel administration, labor relations, banking, theory: Man, 36, married. Experienced teacher or administrator; 12 years of experience in leading universities and business firms. Research, publications; government, and business consultant. E194

Accounting, labor economics, personnel administration, consumer economics, economic history: Man, 47, married, B.S. (business and finance), M.S. (accounting), Ph.D. (labor economics and business management). More than 23 years of experience as collegiate teacher of business and economics subjects and as labor economist for the U. S. Department of Labor. Available in September, 1953, or earlier. E273

Economic analysis, business cycles, public finance, money and banking, national income, international economics, statistics: Man, 34, married, Ph.D. residence completed. Seven years as economist-writer with research and investment organizations; brief teaching experience; research in Middle East conomy. Seeks research, writing, or teaching position, West Coast preferred. E278

Economic principles, theory, history of economic thought, American economic and financial history, security markets, corporation finance, labor and allied subjects: Man, 39, married, B.S. (*magna cum laude*), 1949, M.A., 1950, all examinations and course requirements toward Ph.D. completed and expects to finish dissertation at New York University by June, 1953. Now on teaching staff of Eastern college. Seeks teaching position starting September, 1953. E318

Statistics, business mathematics, corporate finance, marketing, investments, public finance: Man, 39. Assistant professor at well-known university in the East desires six to eight weeks' summer school employment only. Prefers Southwest or Pacific area. E334

Economic principles, problems, economic history, international economics, development of economic thought, comparative economic systems: Woman, Ph.D. Five years of university teaching experience in Midwest; also research experience and publications. Desires teaching or research position. E380

Money and banking, business cycles, theory of statistics, business statistics, economic theory, history of economic thought, economic history, international economics: Man, 31, Ph.D. Brief teaching and substantial research experience. Desires teaching and/or research position. Available in September, 1953. E382

Business economics, economic theory, business organization, labor and industrial relations, marketing, foreign trade, business and government: Man, 29, married, Ph.D., Stanford University. Now assistant professor, Western university. Desires change for financial and professional advancement. Major publication in prospect; interested in teaching, research, or administrative position. Available in summer or fall, 1953. E404

Economic theory, transportation, finance, government and business: Man, married, Ph.D., 1951. Three years of university teaching and one year of government research experience. Desires teaching or research position. E409

Air transportation, international economics, international organization, history of economic thought, economic history, contemporary economic theory, public finance: Man, 32, B.A., M.A., Ph.D. in economics; 11 years of economic research in several areas of the federal government, including national income, fiscal policy, international economics, and air transportation; currently employed as economist in one of the aviation agencies of the federal government. Desires a teaching position in economics. E420

Transportation, public utilities, business and government, insurance, economic history, theory, other economics and business: Man, 33, married, M.B.A., Ph.D., California. University teaching; government economist; some business experience. Desires permanent teaching and/or research position. Available in spring or fall of 1953. Roy J. Sampson, Box 304, Route 1, Forest Grove, Oregon.

International economics, money and credit, principles, Far Eastern economics, public finance, business cycles, price and income analysis, economics of mobilization, economic theory and its history: Man, 30, married, B.A., M.S., all course requirements for Ph.D. Five years of teaching; 3 years as assistant professor and division head, Western state college. Chief economist, territorial office, stabilization agency, since July, 1951. Seeking teaching, research, or combination. Available in summer or fall semesters, 1953. E422

Transportation, U. S. economic history, labor economics, political economy: Man, 32, B.A. (economics), M.A. in Education, Yale, M.A. (economics), LL.B., Boston, Ph.D. residence requirements completed. Experience in teaching, research, and writing. Seeks teaching or research position. E428

Labor problems, trade unionism, economic principles, economic analysis, social security, money and banking: Man, 29, married, Ph.D. residence and written examinations completed. Teaching experience at two large New York universities. Now employed by federal government. Desires teaching position. Available immediately. E429

Labor, comparative economic systems, current economic problems, economic history, economic and social thought, money and banking, public finance, international trade, personnel and manpower policy: Man, 37, married, M.A., and Ph.D. residence completed at Columbia University. Six years of teaching economics and social science as well as background in adult education; 3 years as editor and research director in the book publishing and public relations field dealing with labor relations; 3 years of personnel and industrial relations work in industry; market and opinion research experience. Interested in teaching, research, writing, editorial and/or administrative positions. E434

Agricultural economics, economic geography, economic analysis, international relations, international administration, foreign area specialist (Eastern Europe, Far East, Pacific Area): Man, 34, married, B.S., Cornell (agricultural economics); working on graduate thesis in underdeveloped area development, American University. Three years in private industry, including 1 year private international organization; 6 years with federal government, presently employed in Washington, D.C. Strong sales background. Interested in administrative or research position, government or private industry; will consider outstanding sales position, particularly export-import field. Willing to relocate; particularly interested in Mountain States or far West. E435

Economic principles, business cycles, public utilities, transportation: Man, Ph.D., California. Broad experience in teaching and as economist with nationally known corporation. Available in September, 1953. E436

Labor law, business law, public control of business, etc.: Man, Ph.D., LL.B., associate professor. Position wanted for September, 1953. E439

International trade, resources, finance, economic principles, etc.: Man, 33, married, B.S. (management), M.B.A. (foreign trade); will receive Ph.D. in June, 1953. Five years of university teaching; 1 year of foreign service (international negotiation); several years of industrial and commercial experience. Desires teaching-research post, with desirable climate and topography. Available in September, 1953. E440

Economic analysis, international economics, labor economics, housing, money and banking, business cycles, theory and history of ideas: Man, 35, married, Ph.D. Five years of teaching experience; 4 years of research in federal government and abroad on Fulbright and other fellowships; presently with European Office, Mutual Security Agency. Desires teaching or research position. E441

Industrial organization and control, economic principles and theory, labor economics, money and banking, theory of business, marketing, business organization and management policy: Man, 34, married, B.A., Penn State; M.A., California; Ph.D., Harvard, dissertation half completed. Five years of teaching experience; 2 years of research experience; 1 year as government economist. Currently at West Coast university conducting specialized course for industry. Desires teaching or research position in economics or business administration. Available in September, 1953. E442

Economic principles, commercial law, history of economic thought: Man, 33, married, B.A., 1950, M.A., Duke, 1952, plus 1 year of course work toward Ph.D. Six years of clerical experience in foreign banking; now teaching at Southern college. Seeks teaching position starting in September, 1953. E443

International economics, money and banking, labor economics, industrial management, economic theory, history of economic thought: Man, 28, M.A. in Economics, Ph.D. expected in August, 1953, University of Illinois. Brief experience in teaching, research, and foreign trade administration. Available for academic or nonacademic position in September, 1953. E448

Social control of business, business law, labor, economic principles, business administration: Man, 26, M.A., J.D., Ph.D. candidate, outstanding record. Books; 6 years of teaching experience in leading institutions at graduate and undergraduate levels; stimulating teacher; interested in writing; excellent references. Faculty member at leading private university. Desires academic or nonacademic employment for summer, 1953; also will consider transfer to top-notch liberal arts college. Interested in opportunities for foreign travel. E449

Labor economics, personnel management, industrial sociology, comparative economic systems, market research: Man, 35, Ph.D. expected in spring, 1953. Seven years of teaching experience; at present head of small economics and sociology department; several years of industrial, market, opinion survey experience; project director of research foundation. Multilinguist; extensive foreign travel and research. Interested in teaching and/or research position. E450

Economic development, public finance, international and regional economics, monetary economics, U. S. economic history, statistics: Man, 33, married, Ph.D., Columbia University. Five years of teaching experience; 2 years as consultant in economic development to territorial government; research, writing, and business experience; foreign work and travel. Available for teaching, research, or nonacademic position. E451

Economic fluctuations, economic system of Soviet Union, economic demography, with special consideration of problem of migrations: Man, 32, doctor in economics at an outstanding European university; 1 year of supplementary study at an outstanding American university. Author of a book. Desires employment as instructor or research worker. E452

Comparative economic systems, history of economic thought, international trade, economic theory, money and banking, principles of economics: Man, 26, A.B., Indiana University; M.A., Columbia University; now completing Ph.D. residence requirements at Western university. One year of teaching experience. Seeks position at Eastern college or university. Available in September, 1953. E454

American economic history, corporation finance, business cycles, economic theory, statistics, economic principles: Man, 27, married, B.A., New York University; M.A., Columbia University; mechanical engineering training, Polytechnic Institute of Brooklyn. Seeks part-time evening position teaching at a college in the New York metropolitan area. E456

Marketing research analysis: Above young man also seeks marketing research analyst position with a consulting engineering firm in the New York area to combine his educational training with his experience as a Social Science Analyst for the Bureau of the Census and marketing director for the Israel Bond Drive in Nassau County. Contacts in both industry and government. Excellent references available upon request from the Placement Office at Columbia University. E456

Economic theory, principles, thought, government and business, welfare economics, money and banking, business cycles: Man, married, Ph.D., Columbia. Ten years of teaching; previous business experience; has written 2 books. Available in September, 1953. E457

Economic principles, growth economics, relation of agriculture to foreign trade, trade restrictions: Man, 34, B.S., M.A., Ph.D. (June, 1953). Five years of service in South America with Department of State (2 years as officer-in-charge of consulates); 5 years of experience in university teaching; 3 years, concurrent with teaching, editing fortnightly publication. Interested in economic research for business firms or university teaching. Available in July, 1953. E458

Economic theory, cycles, labor, business and government, economic systems, fiscal policy, economic history: Man, 28, married, Ph.D. Since 1951, 1½ years of independent research financed by federal government, private organization, and university; 2 years of teaching in large university; publications in major economic journals plus two full-length monographs. Currently assistant professor at large university but seeks teaching position in different geographic location. E459

Money and banking, labor relations and personnel administration, American industry and economic geography, business organization and management, business cycles, principles of economics, principles of marketing, foreign trade: Man, 30, married, B.A., M.B.A.; Ph.D. course requirements completed at New York University (Graduate School of Business Administration). Majors in economics, marketing, management, and banking. Six years of college teaching plus practical business background. Now assistant professor of economics at a large Southwestern college. Interested in position as teacher in administration or with business. Available in fall, 1953. E460

History of economic thought, comparative economic systems, public finance, international economics, American contributions to economic theory: Man, 31, married, M.A.; expects to complete Ph.D. at University of Colorado, August, 1953. Six years of university teaching experience. Seeks teaching position. Available in September, 1953. E461

Economic geography, economic history, economic policy, international trade, industry (especially location) studies, economic development, European, Asian, African affairs: Man, 38, married, Ph.D. Six years of graduate and undergraduate teaching experience; also research and writing; now teaching in Eastern university. E462

Economics, banking, investments: Man, 29, B.S., M.S. Five years of experience in bank supervisory agency. Desires position with financial institution in research or investments; also interested in teaching position. E463

Money and banking, investments, corporation finance, business administration, economic principles, international economics: Man, 35, B.S. (business administration), M.A. (economics), plus one year of course work toward Ph.D. Five years of experience in commercial banking; 5 years in insurance planning and counseling; 1 year of teaching experience at outstanding Midwestern university. Stimulating teacher, interested in writing; extensive public speaking experience. Desires teaching position in economics or business administration; prefers Southwest (Texas excluded) or West Coast. Will consider other areas as second choice. Available in summer, 1953. E464

International trade, economic analysis, business cycles, money and banking: Man, 33, married, M.A., Ohio State University. Four years of experience in analysis and research with government agencies. Seeks position as teacher, consultant, or economic analyst. Available immediately. E465

Public finance research and/or administration, monetary and fiscal policy, business economics: Man, 28, married, M.S., near Ph.D., Cornell University. Three years of industrial experience. Available immediately. E466

Economic analysis, industrial organization and control, antitrust, statistics, comparative systems: Man, 28, married, S.B. (*cum laude*), Harvard University; Ph.D. thesis partially completed, Columbia University. Four years of teaching experience in New York City area; some business research experience. Wants research or teaching or combination. Available immediately or fall, 1953. E467

Investment analysis, auditing, budgeting, business finance: Man, 49, Doctor of Law and Jurisprudence. Twelve years of experience in banking, credit, and investment in Europe; 5 years of auditing in large European corporations; 5 years as professor of accounting in an American university. Interested in a position as investment analyst or controller. E468

International economics, money and banking, corporation finance, economic history, accounting: Man, Doctor of Economics (Berlin). Research work with government agency; publications in nine countries; 7 years of college teaching, European and American experience. Desires position in New York area only. E469

Accounting: Man, 36, B.S. (accounting), 1950, M.B.A., 1952, C.P.A. (California), 1953. Two years of public accounting in medium-sized Los Angeles firm; 5 years of government purchasing experience. Will consider offers involving research, writing, teaching, or accounting. Prefers Midwest or South. Available immediately. E470

International economics, trade and banking, European area specialist: Man, married, LL.D., Ph.D. Resident in Canada for three years; willing to relocate. Experience in diplomatic and civil service, top-level international finance, industry, particularly export-import field, research, writing, broadcasting, lecturing. Seeks teaching or research position. Available in September, 1953. E471

International economics, underdeveloped areas, transportation, economic systems: Man, 45, married, M.A. in June. Twenty years of practical experience in foreign trade; state examination in insurance. E472

Labor economics, government and business, fiscal policy, economic theory, principles, political science: Man, 31, married, B.A., M.A.; completed course requirements for Ph.D. Two years of teaching experience in small liberal arts college; 2 years of teaching in a university. Now on the faculty of a New England university. Desires teaching position starting in September, 1953. E473

Economic analysis, international trade and finance, money and banking, public finance, statistics: Man, 36, married, Ph.D. Eleven years of experience in research and consultation for federal government agencies, Federal Reserve Bank of New York, foreign governments, and central banks (Guatemala, Burma, Formosa); 9 periodical publications in academic and semiacademic journals. Social Science Research Council Fellow, 1947. Desires research or advisory position. E474

Marketing, statistics, banking and finance, business research: Man, 31, M.B.A.; Ph.D. residence and examinations completed. Three years of teaching experience. Presently with consulting firm. Interested in college or university position in teaching and/or research; prefers Midwest. Can be available in September, 1953. E475

International economics, price theory, national income, business cycles, money and banking, history of economic thought, elementary statistics: Man, 33, married, B.A. (Phi Beta Kappa), M.A., Yale University; Ph.D., London School of Economics. Brief teaching experience at major university; article in *Economica.* Desires teaching or research position. E476

BACK NUMBERS WANTED

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The Association will pay fifty cents a copy for the following issues (sent prepaid and in good condition) of the *American Economic Review:* March, 1934, supplement; March, 1940, Supplement; September, 1941; September, 1942; March, May, June, 1948; March, June, 1949; March, 1950.

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Send to

Dr. James Washington Bell, Secretary

American Economic Association

Northwestern University, Evanston, Illinois